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ON CERTAIN PECULIARITIES OF THE KNEE-JERK IN SLEEP IN A CASE OF TERMINAL DEMENTIA.

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In the more precise investigations of the physiological conditions modifying the knee-jerk all investigators have found that the mental condition of the normal subject entered largely as a disturbing factor, the different emotional states and the varying conditions of the nervous system invalidating the result to a certain extent, or at least rendering necessary an enormous number of observations before the "normal" knee-jerk for any individual could be obtained. Emotional states being almost completely absent in cases of advanced dementia it was thought that an investigation of the knee-jerk in this state might yield some results on the conditions modifying it. An excellent subject was found in an elderly man, admitted to the asylum in 1841, and with the exception of the seven years preceding 1850 having spent this whole period at the McLean Asylum.

In 1843 it is stated that his "mental faculties [are] mostly gone; never makes an inquiry; spends his time in wandering about the yard; slovenly in his dress;" and but little or nothing could be added concerning his mental condition at the present time. His dementia is complete, he having absolutely no knowledge of his surroundings, not even knowing his name, is unable to answer questions relevantly, his talk being utterly incoherent. He is good natured and docile and has never made the slightest objection to the experiments, which almost always mean an extra nap for him. In fact he

will go to sleep in almost any position in which he is placed, and remains perfectly quiet and contented. Several demented men were tried, but with the exception of this one they all proved resistive or restless, and were given up one after another. The patient finally selected was a peculiarly good one for a prolonged series of tests on the condition of his nervous system as he was subject to periods of depression and exhiliration, such as not infrequently occur in chronic dements, and a record was made of all his bodily functions, pulse, temperature, weight, blood pressure so far as this could be measured, mental condition, and finally knee-jerk in the hope of finding some change in his bodily condition coincident with the change in his mental state. Unfortunately, from the point of view of gaining any information on this particular point, after passing through one cycle of exhiliration and depression he settled down into a state of comparatively even mental and physical life, and as the observations taken during this cycle would need further confirmation no mention will here be made of them. The original problem, also, the theoretically lesser variability of the knee-jerk in a demented person, gradually changed into another as will appear subsequently, and the present paper will be confined to a consideration of two important points that came out in the course of the investigation.

Apparatus and Method of Experimentation.

After a trial of different hammers that of Lombard[1] was finally adopted, that of Prof. Bowditch[2], which was kindly loaned for an experimental trial not proving strong enough for this patient in whom a rather heavy blow was necessary. The hammer was of cast iron, the handle being an iron rod 20 cm. long, on the end of which a similar piece of iron rod 4 cm. long was fastened horizontally, serving as an axis and supported in an iron frame work. An index on the handle moved over a graduated scale, so that by raising the hammer any given number of degrees the same strength of blow was given. Throughout the experiments the hammer always fell 45°, except in a few instances where the strength of the blow was changed for a short time for some special purpose. The hammer was supported by horizontal rods and clamps to a piece of gas-pipe, serving as an upright support fastened to a firm wooden base that could be securely clamped to the table. The arrangement combined firmness and strength, while at the same time the hammer could be quickly raised or lowered,

[1] *American Journal of Psychology*, 1887, Vol. I., p. 12.
[2] Bowditch and Warren *Journal of Physiology*, 1890, XI, p. 27.

or moved in any direction horizontally, and thus easily adjusted to hang at zero of the scale; when the patient's leg was put in position. When pulled back into position to strike the blow the hammer was held by a large electro-magnet, the support of which was also adjustable. The hammer was pulled back by a cord and released by a simple circuit-breaking key. The blows were always given at 5 seconds intervals, except that the interval was occasionally changed for special purposes, but in the reports of the experiments 5 second intervals are always to be understood unless otherwise distinctly stated. The patient reclined on his right side on a mattress placed on a table, with a firm support extending the length of his back. The left knee was supported on a wooden arm extending from a wooden upright that was firmly clamped to the table, and the left foot was supported in a stirrup hanging from the ceiling allowing free movement of the leg. Attached to the stirrup at the point on which the heel rested was a steel rod 3 feet long, passing backwards and supported on pulleys; on its further end was fastened a thread passing over pulleys and attached to a short vertical steel rod, moving up and down through two brass supports, and suspended from above by a light spring. A cork placed on the rod carried on its side the writing point, a piece of light stiff celluloid, which pressed lightly against the drum of a Baltzar Kymograph. When the blow was struck the foot moved forward pulling the steel rod with it, and this by its thread attachment pulled down the writing point, leaving the record of the full length of the kick on the smoked surface of the paper. There was of course always a slight backward kick which appears in the records above the horizontal line made by the drum revolving under the writing point when this was at rest.

Disappearance of the Knee-Jerk in Sleep and the Effect of Auditory Stimuli.

The disappearance of the knee-jerk in sleep has been previously observed.[1] Its complete disappearance was early noticed in our patient and this is therefore no departure from the normal. As would naturally be expected the patient went to sleep much more easily than a person with an active mind, and it was thus possible to observe the phenomena a large number of times.

The behaviour of the knee-jerk when sensory stimuli are received by the patient during this period of sleep appears,

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[1]Bowditch and Warren, loc. cit. p. 59.
Lombard, loc. cit. p. 53.

however, to be a distinct departure from the normal, and suggests that the weakened state of his brain permits the sensory stimulus to exert its effect in reinforcing the knee-jerk for a much longer period than in a person with a sound brain.

That the knee-jerk is increased when the patient clinches his hand or makes any violent movement coincident with the blow was first shown by Jendrássik[1]. "He also thought that stimulation of the sensory nerves had a similar influence on the tendon reflex, but considered his experiments on this point incomplete and that such an influence was more difficult of determination." (Quoted from Bowditch and Warren.)

Mitchell and Lewis[2] made a study of the conditions under which the knee-jerk is increased and diminished, and found that volitional acts directed to other parts of the body, painful stimulation of the nerves of the skin either by pinching or by the application of heat, cold or electricity, caused a reinforcement, as did also a burning magnesium wire exposed to the eyes.

Lombard found that sensory irritations, voluntary movements and strong emotions when synchronous with the blow increased the knee-jerk. The investigation of Bowditch and Warren had for its object a study of the exact relations in time between the knee-jerk and the reinforcing act, and was suggested by the statement of Mitchell and Lewis "that the muscular action or circuit closing, must precede the tap, in order to reinforce it, by a period which is, as yet, undetermined." The conclusion of Bowditch and Warren with regard to auditory stimuli was that "the effect of a sudden auditory stimulus on the extent of the knee-jerk was, in the three subjects of experiment, almost wholly positive, though great individual differences were observed. The maximum effect was produced when the interval between the sound and the blow was 0.2″—0.3″."

As the results here to be recorded have to do with the knee-jerk in sleep the experience of previous investigators on this point is of interest.

Bowditch and Warren[3] found that "the monotonous character of the experiment was often found to produce a decided tendency to sleep in the individual experimented on. To counteract this tendency and to insure a certain degree of attention to the phenomena, the subject of the experiment was required to declare after each knee-jerk whether or not a

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[1] Jendrássik: Beiträge zur Lehre von den Sehnen reflexen. Deutsches Arch. f. Klin. Med. 1883, XXXIII, 177.
[2] *Medical News*, Feb. 13 and 20, 1886.
[3] Loc. cit. p. 58.

sensory-stimulus (i. e. sound, flash, etc.,) had been perceived, or in other words, whether the knee-jerk was normal or reinforced. In spite of this precaution the tendency to sleep was sometimes quite irresistible, and in eight or nine cases the experiment was continued after the subject had yielded to it and was sleeping soundly. It was then found that the knee-jerks, both normal and reinforced grew gradually smaller, and when sleep was profound, disappeared entirely, the blow upon the knee being absolutely without effect. This result is not what might have been expected from our knowledge of the effect of sleep on the ordinary cutaneous reflexes, e. g., that produced by tickling the sole of the foot. Whether this can be regarded as an argument against the reflex character of the knee-jerk, or whether we have here an essential difference between deep and superficial reflexes, are questions to be decided by future investigations."

Prof. Lombard has kindly gone over with me his own curves made in 1887 in New York, generally on healthy medical students, and many of these show an absolute disappearance of the knee-jerk in sleep, and they also show a sudden rise from 0 from some accidental stimulus such as the entrance of a person into the laboratory, but this effect lasted over but a few kicks.

In our demented patient when the knee-jerk has entirely disappeared in sleep an auditory stimulus causes an increase in the length of the kick, and this increase is visible over a long series of jerks. This latter phenomenon is apparently a departure from the normal and a peculiarity of this subject. This in well shown in Fig. 1. The patient was asleep from the beginning of the experiment and soundly asleep with total disappearance of the knee-jerk during the minute and three quarters preceding the time when two light taps were given on the table with the wooden handle of the needle that was being used by the observer to make records on the drum.

The knee-jerk rose from 0 to 9.5 mm.; in 5 seconds rose to 39.5 mm.; in the next 5 seconds to 59 mm.; then fell to 38 mm.; through the 29 succeeding kicks gradually fell with slight fluctuations to 6.5 mm.; remained at practically this length for 3 kicks; rose to 15 mm., and then to 28 mm.; fell to 16 mm. and 14 mm.; then came 5 kicks averaging 6.6 mm.; then came a rise without apparent cause to 44 mm.; then followed a fall to 0 after fluctuations extending over 27 kicks. Thus one auditory stimulus had an apparent effect extending over 72 kicks occupying 6 minutes. During this whole time the subject was sleeping soundly, being apparently as sound asleep as when the auditory stimulus was first given. Five kicks of the value of 0 then followed, and there then

came on another series of increased kicks with no apparent cause, lasting over 16 kicks, with another total disappearance. There then followed 28 kicks with almost total disappearance of the knee-jerk and then two taps (B) brought out the series of reinforced kicks again, with a peculiar series of groups of kicks extending to near the end of the experiment, with occasional total disappearances. Toward the end (C) two taps produced almost no effect, causing a rise only to 10 mm. from a preceding 7, then followed 18, 10.5, 4 and 0. After three of 0 value another lengthened group came on with no apparent cause, and at this point the subject was awakened by being spoken to. It was noted that this day he was decidedly more dull than on the day before, that he slept from the beginning to the time he was awakened, and that all efforts to rouse him at this point amounted to but little as he would simply mutter a little and then drop off to sleep again. There were no accidental sensory reinforcements that were sufficiently noticeable to be brought to the consciousness of the observer who was on the watch to note them. The peculiar set of "groups" of kicks occurring during the course of this experiment will be discussed later on.

This prolonged effect of a sensory stimulus is seen again in Fig. 2, where two clicks were given on a telegraph sounder at A, after the knee-jerk had entirely disappeared. Here the kick rose from 0 to 18 mm., and then to 42 and 44, falling through 17 kicks to 3 mm., not reaching 0 again for 17 more kicks. There were three kicks of the value of 0, a short rise over 4 kicks, a total disappearance during 2 more, the needle not even making a dot on the line, and then there came another of the "groups," B, with no apparent exciting stimulus, lasting over 10 kicks before these entirely disappeared again. There were slight rises above the 0 line, and at C a noise on the floor below caused another series of reinforced jerks, and the subject awoke. Practically this same effect of stimuli is seen in Fig. 3, where at A and B walking on the floor below caused the rises there shown; the much lesser effect of the second stimulus is seen in the fewer number of kicks that were caused by it. Blows were delivered at the regular 5 second intervals although without the effect of moving the needle from the straight line, until at C two taps with the needle-handle and a distant locomotive whistle a little later caused the prolonged series of increased kicks that followed. Here again it must be remembered that to all outward appearances the patient was sleeping as soundly as when the sensory stimuli were received. Fig. 4 shows the same effect once more, the knee-jerk having entirely disappeared during the period represented by the straight

line preceding A, blows being struck at the regular 5 seconds intervals, while at A, a passing barrow caused a slight rise and at B the noise of a passing cart caused a much greater rise, with a secondary rise a little later, with another rise before the knee-jerk entirely reached 0 again, and then after three kicks of the value of 0 there was still another rise, when there was again total disappearance.

In Fig. 5, the effect is shown of giving the rapping-stimulus seven times in succession. The largest effect was after the second time where the reinforcement continued over 13 kicks before the zero point was reached. After the third stimulus there were 8 reinforced jerks before the kicks sank again, not to 0, but to 4 mm. Stimulus no. 4 called out only two reinforced kicks, no. 5 called out 2, no. 6, 4, and no. 7, 6. At no. 8 the patient was awakened.

These would seem to show that the subject had gradually become accustomed to the stimulus and thus its effect was lessened. If it is argued that the patient was more wakeful at 6 and 7 as shown by the fact that the kicks did not return to 0, this would mean also that stimuli did not have so great an effect during this more wakeful condition as during the condition of deeper sleep. This diminution of the effect of a reinforcing stimulus is shown in Fig. 6 where 2 clicks were given on a telegraph sounder at (1), causing a marked effect lasting through 6 kicks, and then disappeared, to be followed by a secondary rise during 7 kicks, and then a total disappearance. A repetition of the clicks however at (2) was followed by no response for the first kick but the second and third kicks rose respectively to 1 and 2 mm., which ordinarily would be accounted for by the jarring of the apparatus, but in this particular instance there had been no movement even from the jarring, so it seems fair to attribute these two movements of the writing needle, slight as they are, to the knee-jerk. A third repetition of the stimulus (3) called out a knee-jerk of 3 mm., followed by a total disappearance during 5 kicks, then a kick of 8 mm. with no apparent preceding stimulus, a disappearance of three kicks, and then a repetition of the clicks, this time three clicks instead of two, produced a kick of 8 mm. with another total disappearance. A repetition of the three clicks (5) caused no response for the first kick, but the second rose to 26 mm., the third to 43, and from this time on there was no disappearance but a kick followed every blow of the hammer.

Attempts to find a similarly marked reinforcement from auditory stimuli while the patient was awake did not meet with as good results as while he was asleep. Fig. 7 shows a portion of a curve taken from the middle of a tracing while

the subject was fully awake. At the places indicated by dots two clicks were given on the telegraph sounder and an apparent reinforcement appears at times, and again an apparent inhibition. This disagreement may be due to the fact that the interval of time by which the sound preceded the blow was not measured, so that the blows may have been struck at the reinforcing interval at one time, and at the inhibiting interval at another. The irregularities in the kicks, during the period that the reinforcing signals were being given, are no greater than the irregularities in the kicks preceding and following the reinforcing signals. The sounds both inside and outside the laboratory, that had so much effect during sleep, never appeared to have a corresponding effect when the subject was awake. To have settled this point definitely would have necessitated a repetition of the elaborate experiments of Bowditch and Warren. Nor was the interval by which the sound preceded the blow in sleep measured; it probably usually varied between one and two seconds. As the peculiar prolongation of the reinforcement made its appearance apparently irrespective of the interval by which the sound preceded, particular attention was not given to this point. It would be interesting to determine if an interval could be found at which the stimulus would inhibit such a prolonged series of kicks as shown in some of the curves. On several occasions the first kick after an auditory stimulus did not rise as high as the second, as shown in Fig. 3, and this raises the question whether the inhibiting interval may not have accidentally been struck here, but the effect of the inhibition passing off during the succeeding 5 seconds, the stimulus exerted its full force and the kick rose to the maximum. Should this be so, it would seem to point to the necessity, in measuring the interval by which the blow must be preceded by the sensory stimulus to produce inhibition or reinforcement, of following the first blow by several more at comparatively short intervals. With regard to sensory stimuli that reached this patient during the time he was awake it can only be stated that these appeared to have a very slight and trifling effect compared with those that reached him during sleep.

It should be added that previous observers of the disappearance of the knee-jerk in sleep and of its rise in this condition in response to external stimuli have not delivered the blows at the same intervals as in these experiments, consequently it is possible that some of the effects of the stimuli may not have been recorded in their tracings. Attempts were made to settle this point by experiments on normal individuals, but the knee-jerk did not entirely disappear in sleep in the trials that were made. In Fig. 12 there is shown a

portion of one of these tracings from one of the medical house-pupils. The experiment was begun at 10.10 p. m. and continued until 11.15 p. m. The portion shown is from about the middle of the tracing, after the subject had become thoroughly drowsy, and a grdual diminuation of the length of the kicks is seen as the experiment progressed. When it became probable that the knee-jerk would not completely disappear the customary auditory stimuli, two taps with the needle-handle, were given at (1), (2), (3) and (4). Although there was a response in each case the effect of the stimuli extended over a much shorter period than in the demented patient. Beginning with the tenth kick preceding the point at which the stimulus was first given at (1), the length of the kicks in millimetres is given below.

5.5	2	2.5	10	7—(4)
5	3.5	9.5	7.5	14
5.5	4	2	3	5
5.5	14	2	3.5	8
3.5	9	2	3	2
10	5	5	3	7
4	10	7	7—(3)	6
4.5	6	15	4.5	10
4	3	4	3	7
15—(1)	5	15	15	9
8	2.5	4	3	6
13	15—(2)	5	4	8
4	3	12.5	2	6
				11

In each of the four cases the two taps were given immediately preceding the kick designated by the figures (1), (2), (3) and (4).

It will be seen that the effect of the first stimulus can be traced over the three following kicks, and then the knee-jerk fell to 4 mm. The second stimulus caused a rise from 2.5 mm. to 15 mm., but at the next kick the knee-jerk fell to 3 mm. The third stimulus caused a rise from 3 mm. to 7 mm., and the next kick fell to 4.5 mm. As the effect of the stimuli was evidently diminishing, the fourth stimulus (4) was made much louder than the preceding ones, and the knee-jerk rose from 2 mm. to 7 mm., and then to 14 mm.; the succeeding three kick were 5 mm., 8 mm., and 2 mm.

In no instance did any such prolonged effect from the stimuli occur as is shown in the tracings from the demented patient. Fig. 12 also shows the "groups" of kicks that could not be identified with any external stimulus; these groups are well shown before the stimuli were given by the two taps. During the time that these groups appeared the subject was in the same condition of half-sleep as when the groups came out best in the demented patient.

It was possible on several occasions to find well marked evidences of this rhythm while the subjects were fully awake. Fig. 11 is a tracing from the demented patient while awake, and the wave like movement of the tops of the kicks is very evident.

As has already been stated there were suggestions of this rhythm (?) all through the tracings, from the first of the experiments, even during the waking state; but in none of the other tracings during the waking condition is it as well shown as in Fig. 11. This same suggestion of a rhythm is also shown in Fig. 13, which is of a tracing taken from a case of well advanced general paralysis. This patient was in the quiet and apathetic stage of the disease, much demented, with ataxic gait, and slow, stammering speech. He was awake during the whole experiment. The same wave-like appearance of the ends of the knee-jerks is well marked.

A tracing from still another patient is shown in Fig. 14. This man was a case of dementia, but not nearly so far advanced as the first case. He was awake during the experiment. There is seen the same suggestion of the wave-like motion of the ends of the knee-jerks.

A point not immediately connected with those already discussed, but having a bearing on the general question, is illustrated in Fig. 15, which is a portion of a tracing from the first case of dementia. The subject was fully asleep as is seen from the total disappearance of the knee-jerk during the first part of the tracing shown. At R the hammer fell out of time, interrupting the regular 5 seconds interval, and as a result of this disturbance of the regularity of the blows the knee-jerk rose during the next four kicks. At R^2 the rhythm of the blows was intentionally interrupted, with the result of causing a rise in the knee-jerk again, but this time less than at first, showing that the nervous system had become accustomed to this change of rhythm. At R^3 the blows were delivered in as quick succession as possible, causing a much greater increase of the knee-jerk than on either of the other two occasions; after eight blows given at the regular 5 seconds interval the knee-jerk again sank to 0.

Rhythmic Grouping.

Besides showing the peculiar prolongation of the effect of a sensory stimulus Fig. 1 shows also the peculiar "groups" of kicks that appear in the curves with no apparent auditory stimulus to account for them. One is tempted to speak of these groups as falling into a rhythm, but they do not occur under circumstances justifying one absolutely in making this claim. Yet looking at this curve as a whole it is difficult not

to think that there must be some rhythmic periodical activity of the body to produce these wave-like rises and falls. It will be seen that there were four of these groups before the auditory stimulus was given at A and the blows at the begining of the experiment appear to have begun at the top of one of these crests. After the effect of the stimulus given at A had disappeared two of these groups came on before the second stimulus was given at B, and after the effect of this had disappeared the groups continued to appear to the end of the experiment. It is to be noted in this connection that the reinforcement at A came at about the time when a "group" might have been looked for, and the question arises did the auditory stimulus simply intensify one of these periodical rises in the knee-jerk. At none of the places where these rises came on, except at A, B and C were there any reinforcing stimuli to account for them. At B also the stimulus appears to have come at about the time when a rise was due, while at C the stimulus seems to have come between two groups, and this may account for the fact that there was a shorter effect of the stimulus here than at A and B.

In examining the remaining diagrams showing the prolonged effect of sensory stimuli to see if one of these "groups" may have entered as a disturbing factor the only instance where this could be thought to do so is in Fig. 4 where the secondary rise after B may possibly be looked on as a "group," but the prolonged reinforcing effect of the stimulus at B is nevertheless sufficiently well marked. The "grouping" is again well shown in Fig. 8. The series is taken from the middle of a tracing after the patient was fully asleep. There was an interval of seven minutes between the first kick and the preceding one, and the first blows after this interval of rest naturally acted as a reinforcement, but the kicks soon diminished, and the peculiar wave-like movement of the curve developed. There were no sensory reinforcements from sounds and the subject seemed to be in the soundest sleep.

The same phenomenon is seen again in Fig. 9, where there were no sensory stimuli except at 1 where two taps were given, and at C where a passing train of cars was heard. This shows also that the two taps did not invariably call out the prolonged reinforcing effect that usually followed.

It was possible on some occasions to account for the changes by variations in the depth of sleep, as in Fig. 10. Here at the points marked A there were no auditory stimuli to account for the rises, but in the intervals, at the points marked B, there were audible evidences of the soundness of the sleep. Usually however it was not possible to detect such a change, and the waves rose and fell without any noticeable change in the regularity of the respiration.

The question as to the cause of this rhythmic (?) grouping of the knee-jerks in sleep is a very interesting one, and considerable attention was given to it in attempting to find an explanation. The phenomenon was noticed very early in the experiments, and many of the tracings made while the subject was awake suggest this same tendency to a periodicity. The "grouping" always shows best, however, in a condition that might be termed half-sleep, where the subject is to all appearances sound asleep, but that he is not in the profoundest sleep is shown by the fact that his knee-jerk is not entirely abolished. Attempts were made to find some connection between these "groups" and the respiratory rhythm, and the respiratory curve and the knee-jerk curve were taken simultaneously, but the results were entirely negative, no particular length of kick being found associated with a particular phase of the respiratory curve. The attempt was also made by taking a plethysmographic tracing from the arm, to find some connection between the depth of sleep and the variations in the knee-jerk, but these at first were equally unsuccessful. Later it was suggested that there might possibly be some connection between the groups and the Traube-Hering curves, and acting on this suggestion additional plethysmographic tracings were taken. A glass plethysmograph, suspended from the ceiling to allow free movement, was placed on the patient's left arm, and connection was made through a glass tube having rubber joints with a very sensitive Marey tambour, the writing-needle of which was placed directly over the writing-needle of the knee-jerk apparatus, so that the two curves were made synchronously on the revolving cylinder of the kymograph. The Traube-Hering curves did not always appear, and there were also many times when the peculiar "groupings" did not appear, as it was necessary for the patient to be in the condition of half-sleep already alluded to, and also that there should be few or no disturbing noises. The necessary conditions have been fulfilled on repeated occasions, however, and a series of tracings obtained where there is a good Traube-Hering curve and also a good series of "groups" of knee-jerks. A portion of one of these double curves is shown in Fig. 16. The pulse beats are well marked and the respiratory rhythm is also well shown. The jarring of the blow of the hammer was sufficient to set the needle of the Marey tambour violently oscillating, so that the respiratory rhythm appears to be unduly accentuated, but this sharp upward rise is due to the vibration of the needle. The Traube-Hering curve is also well marked, the tops and bottoms of the waves being connected by straight lines. It will be seen that there is an

apparent coincidence between the two curves—that the Traube-Hering curve descends lowest in that part of the "group" where the kicks are longest, and at places where the Traube-Hering curve is highest the knee-jerks are much diminished. A rise in the Traube-Hering curve means of course increased blood pressure in the arm, and a fall in the curve corresponds to diminished blood pressure. On Mosso's theory that increased blood pressure in the extremities means lessened blood pressure in the central nervous system we should have, during the time that the Traube-Hering curve is at its height, relative anaemia of the brain and cord; and during the time that the Traube-Hering curve is lowest relative hyperaemia of the brain and cord. There are objections to Mosso's theory, however, as it fails to take into account the abdominal circulation, and the possibility that a change of blood pressure in the extremities may mean simply an opposite change in the abdominal cavity and not in the central nervous system.

Could we adopt Mosso's view it would simplify the problem greatly to say that if we got a rise in the Traube-Hering curve this would mean a diminished blood supply to the brain and cord, and a fall in the Traube-Hering curve would mean a corresponding increase of the blood supply of the central nervous system. The diminished knee-jerk would then naturally follow from the lessened functional activity of the spinal cord at the height of the peripheral Traube-Hering wave, while an increased knee-jerk from increased functional activity of the cord would follow at the low phase of the peripheral Traube-Hering curve. The occurrence of the high phase of the Traube-Hering curve with a diminished knee-jerk, and of the low phase with an increased knee-jerk has been noticed with sufficient frequency to give considerable probability to the theory that there may be a constant relation between the two. The Traube-Hering curves shown in Fig. 16 demonstrate that there is a rhythmic rise and fall in the blood pressure of the arm, as has been frequently proved before. There is thus naturally good reason to infer that with this rise and fall occurs throughout the whole vascular system, and that the vascular supply of the central nervous system is subject to this same periodicity. It also seems perfectly fair to assume that this rhythm might not necessarily be the same throughout the whole vascular system of the body. We only need assume that the vaso-motor centre in the medulla sets up the rhythmic contractions and dilatations in the vascular system that show themselves in the periphery in the Traub-eHering curves, but this rhythmic influence would not necessarily propagate itself throughout the whole

body within the same time, the vascular system possessing considerable inertia, and the amount of blood to be influenced being so great. Several rhythms differing in time might easily be present in the vascular supply of different portions of the body, dependent on the different rates at which the vaso-motor influence had propagated itself through the arterial system; such rhythms should show the same general characters with regard to rise and fall. Fig. 17 shows the result of assuming that this rhythm in the central nervous system differs a little in time from the rhythm in the peripheral circulation. In Fig. 17 the Traube-Hering curve shown in Fig. 16 has been moved back a distance representing 20 seconds in time, which would mean that the vaso-motor influence affected the blood supply of the central nervous system 20 seconds before it reached the arteries of the periphery, which seems a not improbable supposition. Although even with this change, the crests of the Traube-Hering wave do not absolutely coincide with the points of the greatest diminution of the knee-jerk curve, and vice-versa, yet the coincidence is much more striking, and additional weight seems to be given to the inference that there may be some connection between the two curves. It will be noticed that at one point of the knee-jerk there was a disturbing factor caused by the slamming of a door below, at A, sending the knee-jerk up at this point, and thereby apparently making the summit of this knee-jerk wave farther along than it otherwise would have been.

In Fig. 18 there is a much closer coincidence in the two waves than in Fig. 16. Here the Traube-Hering curve makes a long descent between 4 and 5, with a still longer ascent to 6. Again it is to be noted that at 4, where the Traube-Hering wave descends lowest, the knee-jerk curve is also longest.

The same general coincidence of the two curves is again seen in Fig. 19. This was one of the earliest tracings, and the writing needle of the Marey tambour, connected with the plethysmograph, did not make as good a record as it did later. At the end of the record the patient fell into his deepest sleep with the entire disappearance of the knee-jerk. It is interesting to note that, although the Traube-Hering curve continues after the knee-jerk has entirely disappeared, yet at no point does the Traube-Hering wave descend as low as at A, where there was the longest group of kicks. As in Figs. 16 and 17, there is the same number of waves in the knee-jerk curve as in the Traube-Hering curve.

Fig. 20 is from another of the early tracings, but it serves to show the same general characteristics that have been brought out by the other curves. At A a secondary wave appears on the long descent between 6 and 7, and slight cor-

responding changes may be noted in the knee-jerks below.

All the plethysmographic tracings given were taken from the left arm. As it would be valuable confirmatory evidence to obtain similar tracings from the leg, a tin plethysmograph in the shape of a boot was made, and in this the patient's right foot and leg were placed; the rubber band that served to keep back the water coming just below the knee. (Into each plethysmograph water of 30° C. was poured to take up the extra air space not occupied by the leg and arm. The water did not quite fill the plethysmographs, a small air space being left at the top to allow free access of the air to the opening communicationg with the glass tube and Marey tambour. A much better tracing was made by the writing needle with the water in the plethysmograph than with this empty, as the pulsations were thus confined to the small body of air directly above the water, and the arc of vibration of the needle correspondingly increased). The leg plethysmograph was suspended from the ceiling in the same way as that for the arm, and it was still possible to have the left leg, which was still the upper one, in the same position as before for the knee-jerk experiments. Tracings were then made showing synchronous right leg and left arm plethysmographic curves and the left knee-jerk curve. It was found, however, that the jarring of the blow of the hammer on the left leg communicated itself through the bony pelvis sufficiently to affect the right leg in the plethysmograph, causing a serious vibration of the needle with each blow, interfering with the production of a good curve. The arm and leg plethysmographic curves were then taken without the knee-jerk curve with better results, and a general correspondence was found. The leg plethysmographic curve was never as satisfactory as that of the arm, for it was possible to place the whole arm in the plethysmograph, the strong pulsation of the radial artery being strong enough to give a well-marked tracing, but no similarly strong pulsation could be obtained from the foot and calf of the leg. While, therefore, the experiments with the leg plethysmograph need to be carried farther, yet so far they point to similar results as with the arm.

Should the conclusions suggested by the knee jerk and plethysmographic curves seem to be justified, and should they be borne out by further research, the knee-jerk would thus be brought into connection with the other rhythmical and periodic activities of the body. The vaso-motor influence that produces the Traube.Hering curves is necessarily constantly active, but its effects are usually obscured by many other conditions. It would appear probable on *a priori* grounds that the Traube-Hering curve would come out more clearly where

the cerebral influence was removed or inhibited, and we find the Traube-Hering curve coming out with marked distinctness in our demented subject. For the same reason we should expect any phenomenon associated with the Traube-Hering curve also to come out better in such an individual than in a normal subject, and so we find the "groups"—if the relation to the Traube-Hering is a true one — coming out in this same patient. As the Traube-Hering curve is constantly influencing the normal respiratory rhythm, may we not also assume that the Traube-Hering knee-jerk curve, if it is permissible to call it such, is also constantly influencing the knee-jerk? This would explain the mysterious "rhythm" that has seemed to be present in many of the earliest knee-jerk curves taken in this patient, even when awake. We are led from this to a consideration of the knee-jerk of normal individuals, and if the inferences as to the influences affecting the knee-jerk in this demented man are legitimate, it is not evident why the same inferences do not apply to the normal subject. If this is the case then the original point of the investigation no longer has any bearing,—that is, the question as to the theoretically lesser variability of the knee-jerk in a demented person than in a healthy individual; the tops of the knee-jerks of a dement forming theoretically a straighter line than in a sound person. The "normal" knee-jerk curve, therefore, could no longer be considered as theoretically a straight line, but as a true curve correspondingin general with the Traube-Hering curve. It must be admitted at once that it is extremely doubtful if this can ever be shown on a normal individual with the constantly varying emotional condition of healthy persons; nor does it seem scarcely more likely that a normal individual will show such curves even in sleep as are seen in this patient, for the reason that the cerebral influences in a sound person would probably mask this ebb and blow; but this is mere conjecture and must be submitted to actual experiment.

It remains to add that if the "nervous force" or "irritability" of the spinal cord is really subject to this rhythmic action, the question is at once raised if the higher cerebral activities, especially the attention, are also subject to a similar rise and fall, for if the rhythm (?) already described be really due to a vascular process of vaso-motor origin, this same influence must affect also the functional activity of the brain itself.

Conclusions.

The chief interest in the results brought out in this paper lies in the fact that the experiments were conducted on a person whose mind has been weakened by dementia of many years

Fig. I

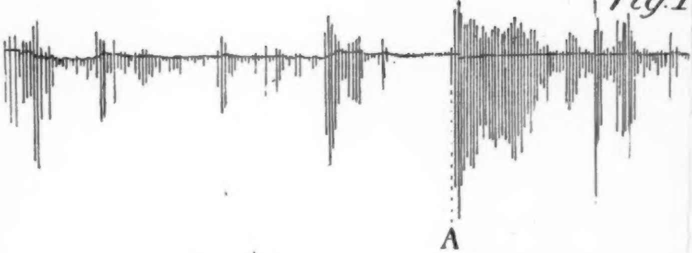


Fig. III

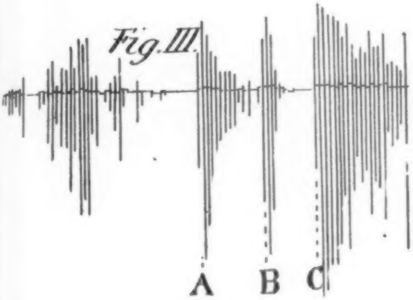


Fig. IV

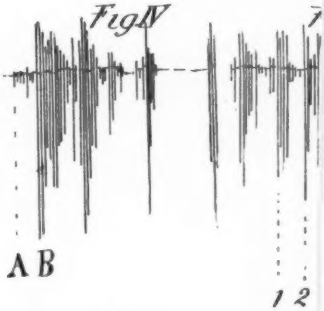
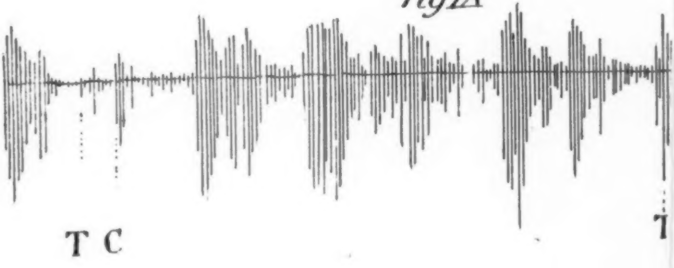


Fig. IX



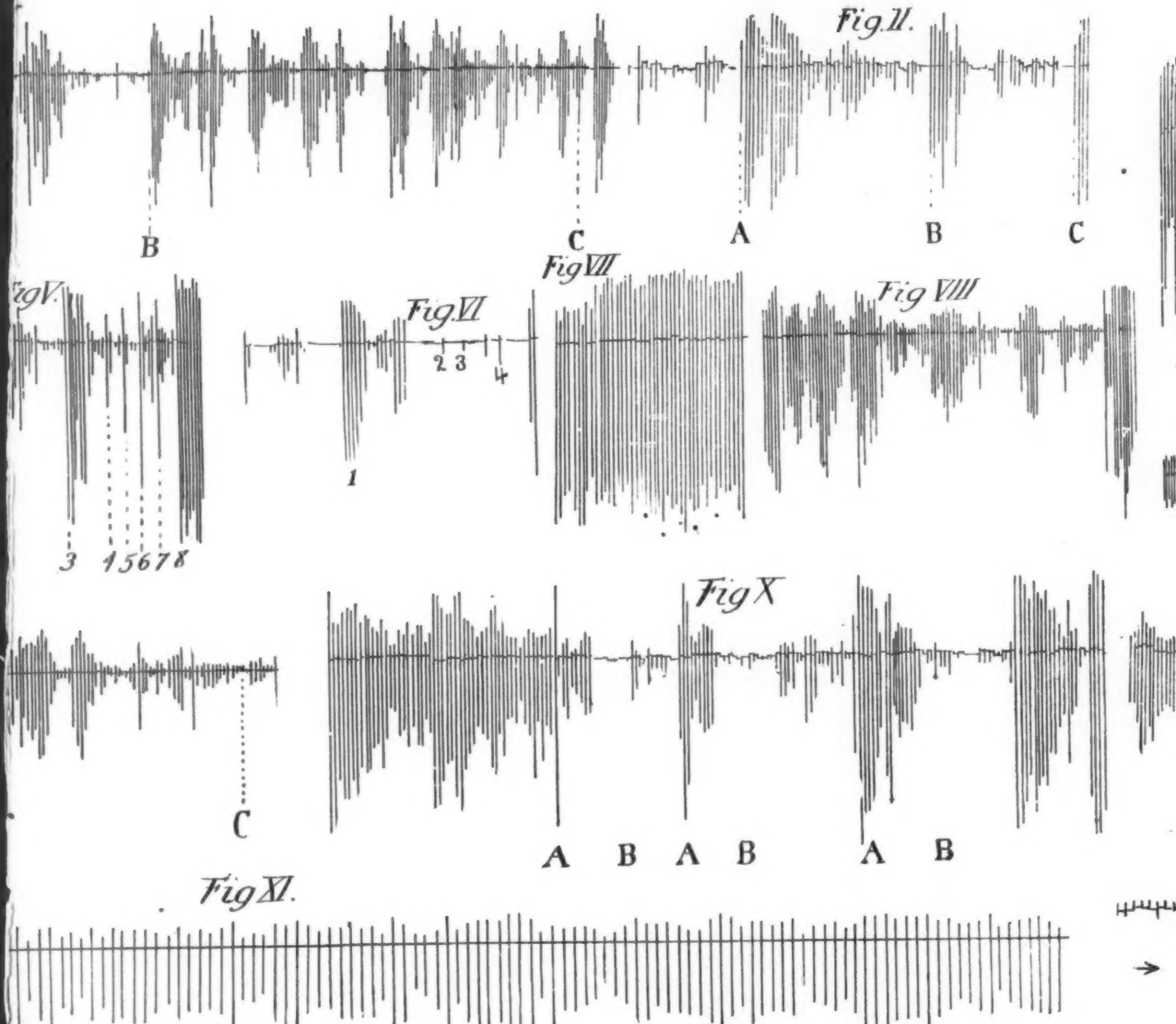


Fig. II.
B
C
A
B
C
Fig V.
3
1 5 6 7 8
Fig. VI
1
2 3
4
Fig VII
Fig VIII
Fig X
C
A B A B A B
Fig XI.

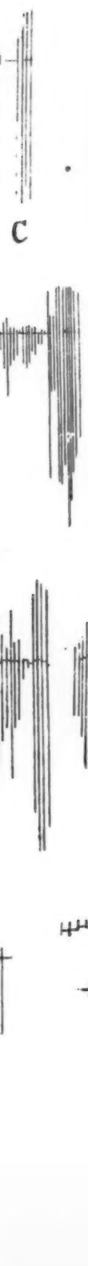


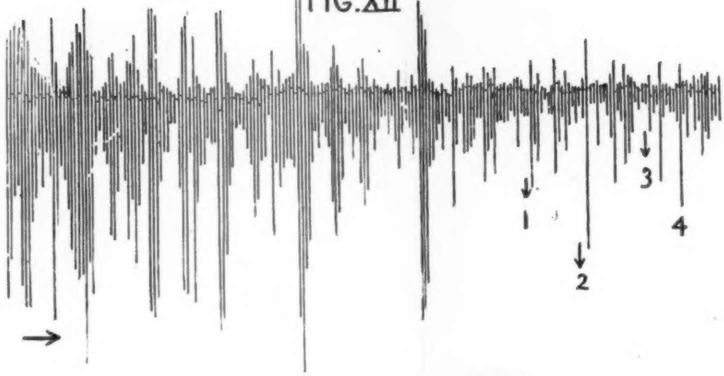
FIG. XII


Fig. XIII.


Fig. XIV.

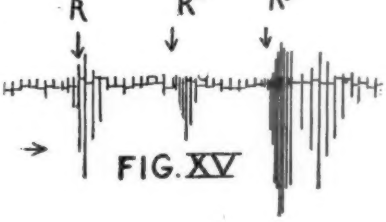



FIG. XV

FIG. XVI

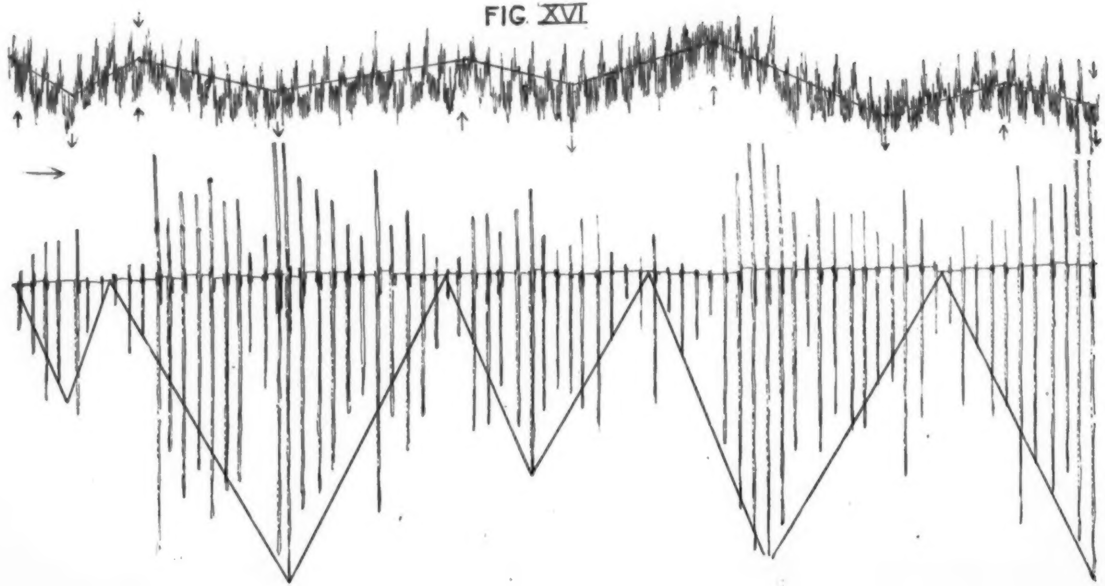
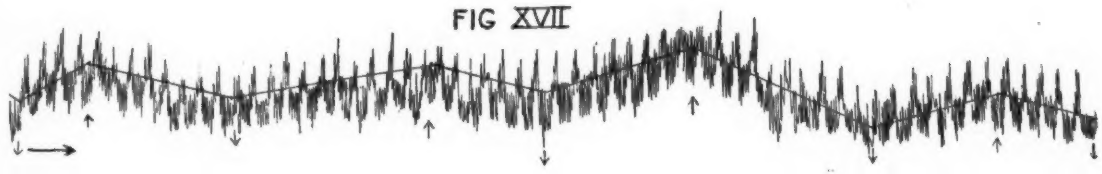


FIG XVII



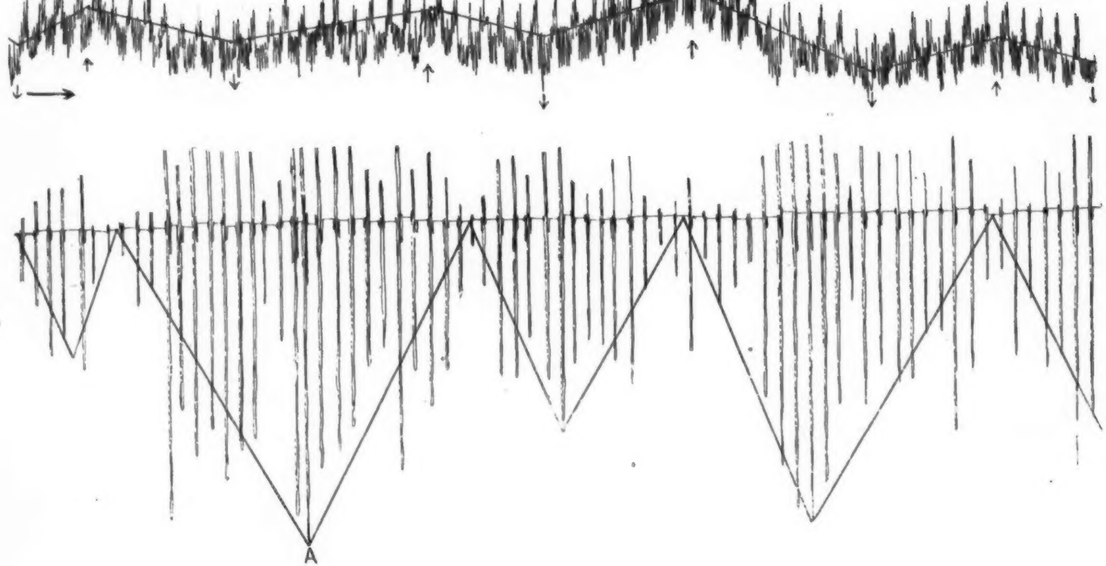


FIG. XVIII

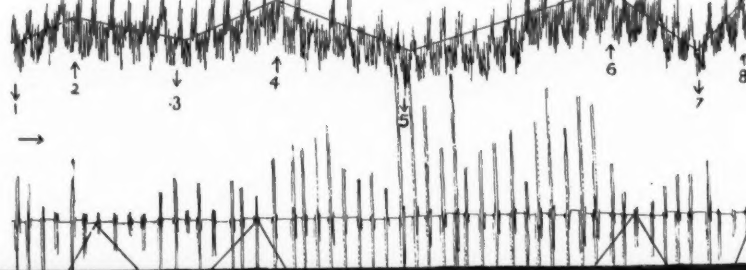


FIG. XVIII

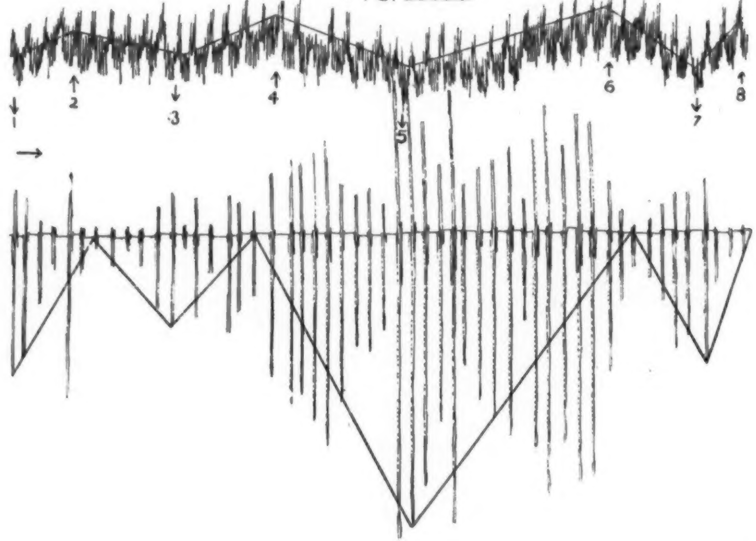


FIG. XIX

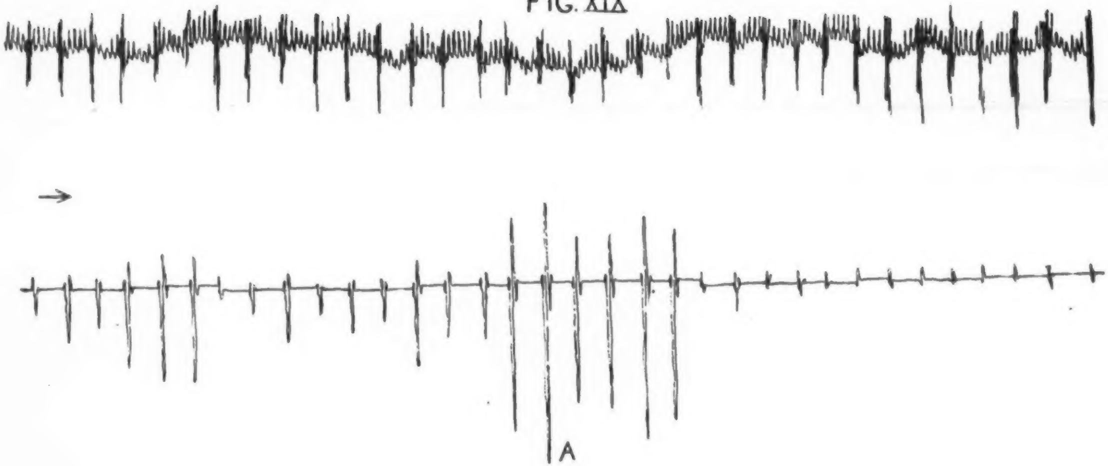


FIG. XX

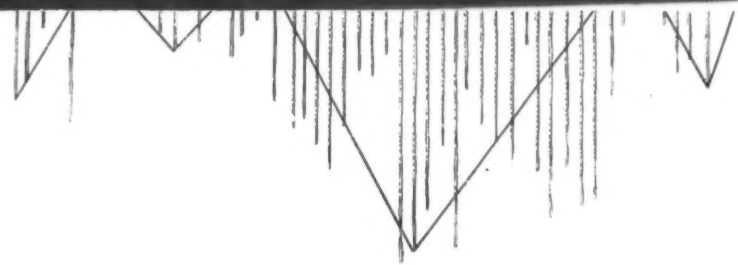


FIG. XIX

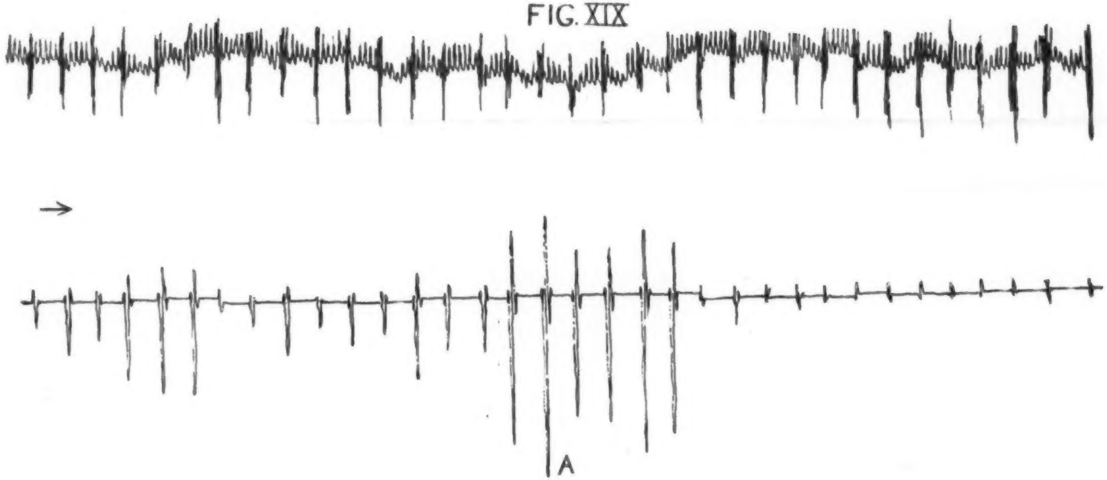
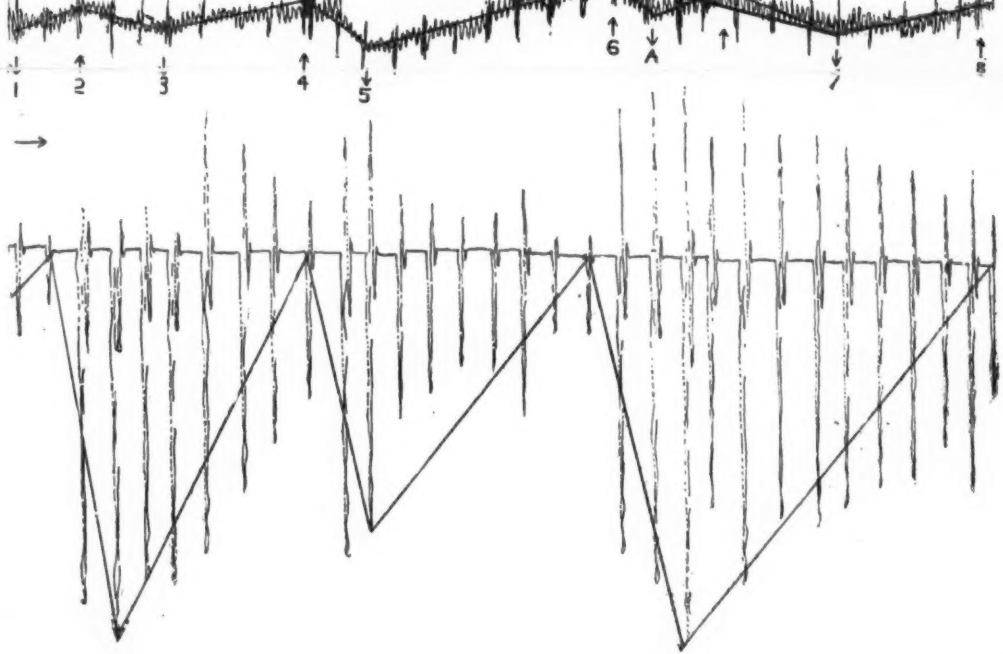


FIG XX



duration, and that this individual shows a greater susceptibility to sensory stimuli than persons in health. The most reasonable explanation of this seems to be that there has been a weakening of the inhibitory influence normally exerted by the brain over the lower centres. The remarks of Mitchell and Lewis[1] have so much bearing on this point that they may properly be quoted here. In discussing the cause of the increase of the knee-jerk from sensory stimuli, they say "It is very difficult to explain the fact that electricity, sensory impressions, and distant voluntary muscle acts increase the knee-jerk and the response to the muscle blow. If we conceive of a series of inhibitory centres extending from the mesocephalon all the way down the cord, and infer that all the agents mentioned are capable, by more or less paralyzing these centres, of releasing the active reflex groups below them, we shall be able to comprehend that the centre thus set free may, by increasing tone, give to the muscle a suddenly enlarged capacity to respond to the tendon taps or the muscle blow. Nearly all the facts with which we are concerned may be explained by inhibition organs and the effects produced upon them. On the other hand, it is equally conceivable that whenever a sensation reaches the cord or brain or both, an overflow occurs, which shall, by increasing the excitation of the centres, be felt throughout the body, and reinforce any organs chancing to be synchronously otherwise excited from without. Under this view we conceive of the nervous force as not confined entirely to the direct paths between the centres and the muscle to be moved, but as overflowing so as to pass through numerous ganglia, adding a certain small increment to their effects when in a state of such activity as the spinal toning centres must be at all times. The tone centres thus stimulated send out a higher wave of excitability to all the muscles, and if at the time this reaches a muscle, that muscle is being excited by a tap, there is an increased response."

Assuming such inhibitory centres as Mitchell and Lewis describe, these would only be kept up to their full functional activity by the healthy condition of the whole nervous system, so that when these inhibitory centres are under a weakened cerebral influence, as in dementia, they offer less resistance to what Mitchell and Lewis call the paralyzing influence of sensory stimuli. In the normal healthy individual in sleep a sensory stimulus preceding a blow on the patellar tendon produces a rise extending at most over a few kicks. This is certainly the most economical method for the individual, if we consider that it is beneficial that the effect of the accidental stimuli that are continually assaulting the nervous system

[1] Loc. cit. p. 203.

should be prevented from spreading over a wide territory or through a long time. In our demented patient a stimulus produces an effect extending usually over a much longer time, even so long as three minutes.

The experiments appear to give a graphic demonstration of the greater susceptibility to sounds, and all external stimuli, in persons with enfeebled, but not organically diseased, nervous systems. If this susceptibility to long continued effect of sensory stimuli in this patient is really due to weakened cerebral inhibitory power, it seems not improbable that the same effect must be produced in individuals with brains weakened or exhausted from any cause,—not necessarily from insanity. The large class of neurasthenic individuals naturally first occurs to one, and the question arises whether, when such patients "feel every sound in their back," this is not due to the uninhibited propagation of accidental external stimuli to the lower reflex centres, as shown in these knee-jerk curves. If so, the therapeutic corollary to this proposition, the necessity of excluding to the greatest possible extent all accidental stimuli from such patients, is graphically demonstrated in these curves.

The conclusions with regard to the so-called rhythm have already been discussed.

The results of the experiments may be summarized in the two following propositions:

In a case of terminal dementia of many years duration a series of experiments on the knee-jerk tend to show that:

1st. Sensory stimuli received during sleep produce a much greater effect and diffuse over a much longer interval than in healthy individuals.

2nd. In a condition of half-sleep when the patellar tendon is struck by blows of uniform strength at five seconds intervals, the knee-jerks fall into groups, and synchronous plethysmographic tracings suggest that these groups have some connection with the Traube-Hering curve.

If the truth of the second proposition can be conclusively established, several important corollaries would seem to follow. These are here stated as facts for the sake of presenting definite propositions, the truth or falsity of which must be submitted to further experimental investigation.

(a) The knee-jerk curve, instead of being theoretically a straight line as has been heretofore assumed, is, in reality, a curved line, with the general characteristics of the Traube-Hering curve.

(b) The spinal cord is not constantly in a condition of the highest potential functional activity, but its activity is represented by a curve of rhythmic vascular contraction and dila-

tation. During the phase of contraction of the spinal arteries, the spinal cord is at its least functional activity, due to a condition of relative anæmia, while during the phase of dilatation of the spinal arteries, the spinal cord is at its greatest functional activity, due to a condition of relative hyperæmia.

(c) The question inevitably raised by (b) is whether the higher activities of the brain are also subject to a rhythmic rise and fall synchronous with vascular dilatation and contraction.

It remains to express my obligations to several who have assisted me in the details of the work, and to extend my thanks to my colleague, Dr. D. H. Fuller, and to Drs. Abbot, Young, Fitz and Sawyer, medical house-pupils, for their assistance. To Dr. Fitz I am especially indebted for assistance and suggestions in regard to apparatus.

THE GROWTH OF MEMORY IN SCHOOL CHILDREN.

By Thaddeus L. Bolton, A. B.

(From the Psychological Laboratory of Clark University.)

During the Spring of 1891, by permission of the School Board in Worcester, Mass., Dr. Franz Boas of Clark University took certain anthropological measurements of the pupils in the Grammar Schools, and also made certain tests of eye-sight, hearing and memory. The memory tests, which were made upon about fifteen hundred pupils in the Grammar Schools, above the second grade and below the high school, together with some tests from the Normal School, came into my hands for examination. To complete the material for all the grades in the public schools, the tests were made upon some of the senior and sophomore pupils in the High School.[1]

The Method of Making the Tests.—A series of numbers in which the digits were so arranged that they did not stand in their accustomed order and no digit was repeated, was read before each class to be tested, and each class was tested on four different occasions. In two Grammar Schools and in the Normal School, where the purpose was to determine the effect of fatigue, two of the tests were taken in the morning immediately after school assembled and the other two just before closing in the afternoon. In the other schools the tests were made in the morning. The digits were dictated slowly and distinctly at intervals of about two-fifths of a second with care to avoid rhythm or grouping, and at a given signal after the dictation of each number was finished, the pupils wrote the digits as they remembered them. To avoid a confusion of terms, *observation* will be used to indicate a group of five or more digits; *digits*, to designate the figures; and *place*, to indicate the position or order from

[1] I wish to acknowledge my indebtedness to Dr. Boas for this material and his advice in regard to the method of treating it. At his suggestion I have introduced the theoretical treatment of the curves. I am also indebted to Dr. E. C. Sanford for helpful suggestions, and to the teachers of the public schools who have assisted in collecting this material.

the beginning of the number—observation. In the lower grades of the Grammar Schools and in the High School twelve observations constituted a test, but in the eighth and ninth grades only nine observations were made at each test. In the lower grades the first three observations of each test were made with five digits, the second three with six; the third three with seven; and the last three with eight. In the higher grades and in the High School the first three observations were made with six digits, and in the Normal School with seven. The tests being repeated at four different times, twelve observations with five, six, seven and eight digits respectively, were made upon each pupil, and it would be possible for each pupil to get any number of the twelve correct. An observation was considered correct when only those figures which the teachers had dictated were present in the same order as that in which they had been dictated. The various kinds of errors will be treated further on. When the observations at each test are begun with five-place numbers and gradually increased to eight, the pupils easily grasp the five-place numbers and are led by these to grasp and retain more than they would otherwise be able to do. If the observations are begun with seven-place numbers and only one observation made with the seven-place number before making one with an eight or a nine, the number of figures is not so easily remembered, and more errors result from this cause. When we come to discuss the various classes of errors in the observations on the Normal School pupils, this matter will come up again. In the sixth grade of one Grammar School through a misunderstanding on the part of the teacher, all the observations taken at the first test were made with five-place numbers, those at the second with six, at the third with seven, and at the fourth with eight. After two or three trials the pupils became aware of the number of digits to expect at each test and gave their attention more to retaining the proper digits in their places. As the number of pupils is so small and the results from this grade do not differ more from the results of the next higher or lower grade than the results of some other grades differ from those of the next higher or lower grade, these pupils have been included in the charts showing the curves for ages and grades.

By classifying the pupils of a particular age or in each grade according to the proportion of the twelve observations on five-place numbers that were correct, and the same for six, seven and eight-place numbers, and marking the percentage that each class was of the total number of pupils in the grade or of the age on thirteen ordinates (twelve for the

twelve observations and one for those pupils who had none correct), and connecting these points by a line, a curve representing the distribution of the pupils of the age or grade will be obtained. The maximum of the curve will then show the proficiency of the pupils for the age or grade in remembering five, six, seven or eight-place numbers, as the case may be.

Upon Chart I the curves show the distribution of the pupils according to the ages and upon Chart II the distribution according to grades. As the observations were made with five, six, seven and eight digits, four sets of curves will be found upon each chart. The curves for the grades in which eight-place numbers were used, are found at the top of the chart, and those for seven, six and five-place numbers in order below. Under each curve appear the number of pupils and their ages on Chart I and their grades on Chart II. To give a graphic representation of the value of these curves, which is to show the increasing accuracy with which the older pupils remember a given number of digits, the average percentages of pupils in each grade and of each age, who have got six or more (for five-place numbers, ten or more) of the twelve observations correct have been taken and this average has been marked upon the twelfth ordinate of each curve. When these points are connected in each set of curves a line is obtained, the rise in which, from left to right, will then represent the increasing accuracy with which the older pupils and the pupils in the higher grades remember a given number of digits. This line, of course, is arbitrary, but it will be found to correspond very closely with the probabilities of errors for the curves. A comparison of these lines will show a more uniform rise in Chart I Chart II shows that the eighth and ninth grades fall below the seventh on eight-place numbers; on seventh-place numbers, the fifth grade falls below the fourth, the eighth below the seventh, and the High School below the ninth grade. Other cases need not be mentioned. On Chart I only two actual falls are noticed, and these are less than one per cent. Considering that our tests measures the length of the memory-span, we can conclude that the memory-span increases with the age rather than with the growth of intelligence, as determined by the tests used in promoting pupils from one grade to another. Our tests do not apply to the retentiveness of the memory. They may be considered as tests of the power of concentrated and sustained attention. My own experience and observations upon the pupils while the tests were being made, seem to indicate that most pupils depend upon their powers of visualization to remember the number of digits, and at the

same time they were noticed to repeat the digits as they were dictated. A comparison of the standing of pupils in their grades and their ability to remember figures was undertaken to determine, what was the relation between the memory-span and intellectual acuteness of the pupils. As the pupils depend upon their powers of visualization, this subject becomes more interesting in determining how far this power is of service in school work and how closely the power of concentrated and sustained attention is related to intellectual acuteness. For this purpose the teachers in the Oxford and Freeland Street Schools and of the High School were requested to give an estimation of what they considered was the general standing of their pupils with respect to the school work. The pupils were classed as either good, fair or poor, and these classes were compared with three classes which were determined in the memory test by the proportion of correct judgments. The percentages of pupils, for whom the two methods of ranking gave the same results, are given in the table. The letters of the following table (A representing good, B fair and C poor) in the left hand column indicate the teachers' classification, and those in the upper line the classification by the memory test.

TABLE I.—Giving the comparison of the teachers' estimation of standing of the pupils, and their standing, as determined by the memory tests. The classes of the teachers are represented as 100 and the others are expressed in percentages.

	A	B	C
A	32.6 %	51. %	16.3 %
B	21.4 %	58.2 %	20.4 %
C	24.1 %	49.4 %	26.5 %

Only eight and a half per cent. more of the pupils who were classed A by the teacher have been classed A rather than C by the memory tests. If our tests may be considered as tests of the ability for concentrated and sustained attention and of the power of visualization, we can conclude that these powers are not the only ones concerned in intellectual pursuits and are not sufficient for a successful undertaking of intellectual work. Intellectual acuteness, while more often connected with good powers of visualization and of concentrated attention, does not necessarily require them, and it cannot be said

that those pupils who are bright intellectually are more distinguished on account of their good memories. The fact that a good memory is not necessarily accompanied by intellectual acuteness, adds weight to the conclusion that the growth of the memory does not necessarily accompany intellectual advancement.

Theoretical Treatment of the Curves.—If we consider that the twelve observations made upon each pupil are subject to the law of chance, then we can construct from the probability of error for any curve the corresponding theoretical curve, and if our supposition is correct, the theoretical and actual curves should correspond very closely. In saying that the answers of the pupils are subject to chance we mean that they are just as likely to err on one observation as upon another, and if this is true, we can treat these observations according to the law of probability. The probability of error for any curve is obtained by subtracting the actual number of correct judgments from the possible number and finding what percentage this difference is of the possible number of correct judgments. With this percentage as the probability of error, we construct the theoretical curves according to the formula

$$p^{12}+\tfrac{12}{1}(1-p)p^{11}+\tfrac{12}{1}\cdot\tfrac{11}{2}(1-p)^2p^{11}+\tfrac{12}{1}\cdot\tfrac{11}{2}\cdot\tfrac{10}{3}(1-p)^3p^{10}+ \\ \ldots\ldots+(1-p)^{12}$$

in which p represents the probability of error. When we construct these theoretical curves, which are found in Table II, we find they do not agree except in a few cases with the actual curves. The form, however, is somewhat the same, but the absolute values are different. Either the supposition is not correct and hence this treatment is not possible, or some element has entered in this case which prevents this material from being so treated. The latter alternative is, perhaps, the correct one.

With completely naïve subjects and like conditions it does not seem probable that twelve observations upon one pupil would differ from one observation upon each of twelve pupils; and yet the variations in the probabilities for the different pupils, when classed according to age or grade, is so great that we might not get an exact correspondence between the theoretical and actual curves even with the many observations under the most uniform conditions. As we shall show further on that the children increased in their power to remember figures with each succeeding test and in one school they were not completely naïve with respect to the tests, a possible explanation is found for this disparity between the theoretical and actual curves. This explanation is further strengthened by the fact that in those curves,

where the probablity of error is less than five per cent., there is a close correspondence between the theoretical and actual curves (see Table II.). Where the probability of error is less than five per cent. for the first test, little increase in the accuracy of the judgments took place for the succeeding tests and hence the law will apply.

The actual curves are compounded of the curves represented by the probabilities of each pupil of the age or grade; the percentages of increase with each test show that there was a different probability for each observation. The effect of compounding a curve of several curves with very different probabilities is to broaden and flatten it, and it is just in this respect that the actual curves differ from the theoretical. If we construct the theoretical curves from the probabilities for the four tests on seven-place numbers given in Table VII. and compound these, we get a curve very much flatter than any of the theoretical curves. The absolute values of this curve are 0.2, 1.4, 4.7, 10., 15.3, 18.1, 17.8, 14.5, 9.8, 5.4, 2., 0.5 and 0. The probability for each test represents a curve compounded of three other curves which would have the tendancy to modify further in the same way the curves of which we have given the absolute values. This probably explains the disparity in form between the theoretical and actual curves and in view of the number of pupils represented by each curve the individual probabilities may account for the irregularities.

In Table II. the percentages for the theoretical and actual curves for the three Grammar Schools are given. The first part of the table is taken up with the actual curves, and the second with the theoretical curves. At the top of the table the numbers of the ordinates and the probabilities of error corresponding to each are given. In the columns, below the percentages of pupils for the different ages and grades and for five, six, seven and eight-place numbers are given under each ordinate for the actual curves. In the last column the figures represent the probabilities of error for the curves. For the theoretical curves these values are assumed. To make a comparison of the theoretical and actual curves take the probable error for any actual curve and find the theoretical curve whose probability of error most nearly corresponds to it. If the two curves correspond, the absolute values should agree closely.

TABLE II.—Giving the percentages of pupils and probabilities of error for the theoretical curves and for the actual curves of both the grades and the ages.

PART 1. *Actual Curves for Ages on 5, 6, 7 and 8 Digits.*

Number of Ordinate.		0	1	2	3	4	5	6	7	8	9	10	11	12	Probabilities of Error.
Probabilities of Error for each Ordinate.		% 100	% 91.6	% 83.3	% 75	% 66.6	% 58.3	% 50	% 41.6	% 33.3	% 25	% 16.6	% 8.3	% 0	
No. Digits.	Age.														
5	8 yrs.	0	0	1.	2.5	2.5	1.	4.	6.5	8.	12.	12.	14.5	34.5	20.
	9 "	0	0	2.	2.	0	1.	4.5	2.	4.5	7.5	10.	20.	45.5	14.5
	10 "	0	0	0	1.	1.	2.	1.	2.	6.5	8.5	13.	20.5	45.	12.
	11 "	0	0	0	0	0	0	1.	1.	5.	5.	10.	22.5	53.5	9.3
	12 "	0	0	0	0	0	0	3.	3.	3.	5.	8.5	18.	60.0	7.4
	13 "	0	0	0	0	0	1.	0	1.	1.	1.	17.5	18.5	58.5	6.6
	14 "	0	0	0	0	0	0	0	0	0	4.5	12.	17.	67.5	4.6
6	8 "	6.5	8.	13.	9.	9.	9.	10.5	6.5	6.5	2.5	5.	5.	8.	56.3
	9 "	7.5	6.5	9.5	6 5	9.5	5.5	3.	6.5	7.5	8.5	7.5	14.	9.5	45.5
	10 "	5.5	3.	6.5	5.5	9.5	9.5	6.5	3.	11.	9.5	9.5	15.	5.5	42.5
	11 "	1.	1.	4.	4.	5.	6.	12.	5.	13.	18.	9.5	8.5	15.	32.4
	12 "	1.	2.5	4.	2.5	4.	7.5	8.5	7.	11.5	14.5	8.5	15.	15.	31.3
	13 "	1.5	0	4.5	1.	5.5	2.5	7.	9.5	4.5	9.5	9.5	19.5	25.	27.7
	14 "	0	0	1.	1.	7.	3.	3.	12 5	10.5	11.5	14.5	11.5	25.5	23.5
	15 "	1.	1.	1.	2.5	2.5	3.5	4.5	12.	9.	11.	8.5	19.5	23.	25.3
7	8 "	40.5	16.5	14.5	2.5	0	2.	2.	2.	0	5.	0	5.	5.	78.
	9 "	15.5	8.5	8.5	14.	7.	9.5	7.	7.	8.5	4.	4.	4.	2.5	64.6
	10 "	19.5	13.	10.	8.5	9.	5.5	9.	8.5	3.5	5.5	2.	5.5	1.	66.2
	11 "	11.5	9.5	8.5	8.5	5.	9.5	11.5	7.5	6.5	6.5	6.	4.	4.	62.5
	12 "	12.5	5.	6.5	9.	5.	5.5	8.	12.5	7.5	9.	10.5	5.5	4.5	51.5
	13 "	5.5	5.5	4.5	8.	5.5	13.	8.	7.	5.5	7.	16.5	11.	3.5	49.2
	14 "	2.	5.	8.5	8.5	6.5	9.5	5.	5.	8.5	13.5	11.5	8.5	8.5	36.1
	15 "	9.5	6.5	7.5	6.5	6.5	1.	8.5	6.5	10.5	10.5	9.5	12.5	10.5	40.7
8	11 "	56.	15.5	8.	10.	2.5	2.5	5.5	1.	3.5	2.5	0	1.	2.5	84.
	12 "	23.5	15.5	12.5	5.5	6.5	4.5	8.5	4.5	3.5	4.5	2.5	2.	3.5	73.7
	13 "	25.5	17.	10.5	8.5	5.5	5.5	5.5	4.5	5.5	1.	6.5	4.5	1.	70.5
	14 "	22.5	8.5	12.5	13.5	4.5	5.5	5.5	5.5	3.	5.5	3.	7.5	3.	65.5
	15 "	18.5	12.5	7.	14.5	8.	6.5	7.	11.	4.5	6.5	2.	0	2.5	66 2
Actual Curves for Grades on 5, 6, 7 and 8 Digits.															
No. Digits.	Grades.														
5	3	0	1.	1.5	3.5	3.5	1.	3.5	5.5	10.	12.5	17.5	19.5	23.	21.3
	4	0	0	0	0	0	0	1.	2.5	4.5	6.	11.	19.	57.	6.1
	5	0	0	0	0	0	1.	1.5	1.5	4.5	5.5	16.	24.	46.	8.4
	6	0	0	0	0	0	1.	0	1.5	3.	4.5	11.5	12.5	66.	6.5
	7	0	0	0	0	0	0	1.	0	0	1.	6.	15.	77.	1.6
6	3	11.	9.5	13.5	10.5	12.5	11.5	7.5	4.5	6.	2.5	3.5	3.5	3.5	63.
	4	3.5	4.	4.5	4.5	5.	7.5	5.5	5.5	7.	13.	14.	15.5	10.5	31.9
	5	1.5	4.5	7.5	5.5	9.5	8.	9.5	8.5	12.	11.	8.	11.	5.	43.
	6	1.5	1.	2.5	1.	6.5	7.5	10.5	10.5	10.5	17.5	8.5	11.5	12.5	32.
	7	0	1.	2.	3.5	2.	2.	3.5	4.5	4.5	9.	13.	23.	32.5	16.7
	8	.5	.5	2.5	0	3.	3.	6.	13.	8.5	14.5	13.	15.	20.5	16.6
	9	0	0	1.5	2.5	4.5	4.5	3.	7.	10.5	5.	8.5	22.5	32.	16.5
	H.S.	0	0	0	0	2.	2.	2.	0	6.	6.	8.	30.	46.	9.

Actual Curves for Grades on 5, 6, 7 and 8 Digits.—Continued.

Number of Order.		0	1	2	3	4	5	6	7	8	9	10	11	12	Probable Errors.
Probable Error of each Order.		% 100	% 91.6	% 83.3	% 75	% 66.6	% 58.3	% 50	% 41.6	% 33.3	% 25	% 16.6	% 8.3	% 0	
7	3	46.	18.	15.	4.5	3.	2.	4.5	2.	0	1.5	1.5	0	0	87.7
	4	19.	10.5	11.	8.	4.5	5.5	11.	5.	4.	8.	3.	7.	2.	63.3
	5	19.	11.	6.5	12.	7.	12.	9.5	11.	1.5	1.5	3.5	2.5	1.5	67.
	6	7.5	9.5	7.5	11.5	7.5	6.5	10.5	10.5	6.5	7.5	9.	3.5	2.5	59.
	7	6.	2.	6.	6.	5.	5.5	6.	8.5	9.5	11.	15.5	13.5	5.5	46.4
	8	6.	6.	4.5	8.5	6.	8.5	8.5	8.5	11.	9.	9.	9.	7.5	44.5
	9	3.5	4.5	5.5	3.5	3.5	2.5	4.5	5.5	7.5	13.	15.5	15.5	15.5	32.5
	H.S.	2.	4.	8.	10.	10.	10.	6.	4.	10.	12.	12.	10.	2.	50.
8	6	37.5	19.	11.	7.5	6.5	5.5	.5	1.5	2.5	1.	.5	1.5	0	85.
	7	11.	9.	12.5	13.5	6.5	5.5	8.5	6.	6.	6.	7.	6.	4.5	57.5
	8	22.5	16.	11.	12.5	6.	4.	5.5	7.	5.5	2.5	4.	1.5	2.5	70.
	9	19.	10.	8.5	12.5	5.5	7.	6.	12.	2.5	9.	1.5	2.5	3.	64.8
	H.S.	18.	18.	6.	12.	6.	10.	10.	6.	4.	4.	4.	2.	2.	67.

PART 2.—*Theoretical Curves.*

0	0	0	0	0	0	0	0	0	.3	3.2	22.7	73.8	2.5
0	0	0	0	0	0	0	0	.3	1.7	9.9	34.1	54.	5.
0	0	0	0	0	0	0	.7	1.4	4.6	17.	38.1	39.3	7.5
0	0	0	0	0	0	0	1.5	2.1	8.5	23.	37.6	28.2	10.
0	0	0	0	0	0	.4	1.7	6.8	17.2	29.2	30.1	14.2	15.
0	0	0	0	0	.4	1.5	5.3	13.3	23.6	28.3	20.6	6.8	20.
0	0	0	0	0	1.1	4.	10.3	19.1	25.8	23.3	12.7	3.1	25.
0	0	0	.1	.7	2.9	7.9	15.8	23.1	24.	16.8	7.1	1.3	30.
0	0	0	.4	1.9	5.9	12.7	20.4	23.7	19.5	10.9	3.6	.5	35.
0	0	.2	1.2	4.2	10.1	17.7	22.7	21.3	14.2	6.3	1.7	.2	40.
0	.1	.6	4.3	7.2	14.9	21.3	22.3	17.	9.2	3.4	.7	.07	45.
.02	.3	1.6	5.3	12.1	19.4	22.6	19.4	12.1	5.3	1.6	.3	.02	50.
.07	.7	3.4	9.2	17.	22.3	21.3	14.9	7.2	4.3	.6	.1	0	55.
.2	1.7	6.3	14.2	21.3	22.7	17.7	10.1	4.2	1.2	.2	0	0	60.
.5	3.6	10.9	19.5	23.7	20.4	12.7	5.9	1.9	.4	0	0	0	65.
1.3	7.1	16.8	24.	23.1	15.8	7.9	2.9	.7	.1	0	0	0	70.
3.1	12.7	23.3	25.8	19.1	10.3	4.	1.1	0	0	0	0	0	75.
6.8	20.6	28.3	23.6	13.3	5.3	1.5	.4	0	0	0	0	0	80.
14.2	30.1	29.2	17.2	6.8	1.7	.4	0	0	0	0	0	0	85.
28.2	37.6	23.	8.5	2.1	1.5	0	0	0	0	0	0	0	90.
39.3	38.1	17.	4.6	1.4	.7	0	0	0	0	0	0	0	92.5
54.	34.1	9.9	1.7	.3	0	0	0	0	0	0	0	0	95.
73.8	22.7	3.2	.3	0	0	0	0	0	0	0	0	0	97.5

In Table III are given the probabilities of error for every curve upon Charts I and II. Part I of the table is taken up with the curves for ages and Part II with the curves for grades. These probable errors show the same general results that the line drawn across the curves shows. When the pupils are classified according to their ages, the figures rep-

resenting the probable errors show a more uniform decrease in passing from the younger to the older pupils than from the lower to the higher grades. The High School pupils[1] are not included in the classification for ages. Where the probabilities of error for the higher grade is greater than for a lower, or for older than for younger pupils, the number has been set in heavy faced type.

[1] The Normal School pupils have been purposely left out of this part of the treatment. The tests were not made with sufficient uniformity to allow them to be classed with the public school pupils.

TABLE III.—Probabilities of Error upon 5, 6, 7 and 8 digit series for all ages and grades.

PART 1.—*Probabilities of Error for Ages.*

No. Digits	8 yrs.	9 yrs.	10 yrs.	11 yrs.	12 yrs.	13 yrs.	14 yrs.	15 yrs.
5	20.	14.5	12.	9.3	7.4	6.6	4.6	
6	56.3	45.5	42.5	32.4	31.3	27.7	23.5	**25.3**
7	78.	**64.6**	66.2	62.5	51.5	49.2	36.1	**40.7**
8				84.	73.7	70.5	65.5	**66.2**

PART II.—*Probable Errors for Grades.*

No. Digits	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	H. S.
5	21.3	6.1	**8.4**	**6.5**	1.6			
6	63.	31.9	**43.**	**32.**	16.7	16.6	16.5	9.
7	87.7	63.3	**67.**	59.	46.4	44.5	32.5	**50.**
8				85.	57.5	**70.**	**64.8**	**67.**

In psycho-physical experiments it is customary to take seventy-five per cent. of right answers as the point at which the subject may be safely said to have some knowledge of that concerning which he judges. This standard is chosen for experiments in which a choice is made between two alternatives, where, by mere guesses, the subject will get 50 per cent. correct. In our test the subject must be supposed to have exact knowledge before he can recall correctly any number of digits. Whatever standard we choose, then, for these tests, it must be considered as the probability that a certain

number of digits should be judged correctly every time. If we choose seventy-five—though it seems to me a less figure might be chosen—our tables show that all the pupils below the 6th grade and over thirteen years of age reach the limit of their memory span at six, and all others at seven. Six may then be taken as the limit to the memory span for most Grammar and High School pupils.

Any treatment of a subject of this kind would be incomplete if no comparison were made between the boys and the girls. For this purpose the boys and girls have been classified according to their ages; in order to get classes sufficiently large to form a comparison, it was necessary to put the pupils differing by two years in age instead of one into each class. The probabilities of error have been found for each class and the comparison is made in the following table. The ages together with the probabilities of correct judgments are given for each class.

TABLE IV.—Showing separately the probability of Error for Boys and Girls.

	Boys.	Girls.
FIVE-PLACE NUMBERS.		
Pupils under 10 years	16.	13.7
Pupils over 10 and under 12	10.4	11.5
Pupils over 12	11.5	11.6
SIX-PLACE NUMBERS.		
Pupils under 11 years	47.6	47.9
Pupils over 11 and under 13	35.2	26.5
Pupils over 13	25.4	25.
SEVEN-PLACE NUMBERS.		
Pupils under 12 years	65.6	61.6
Pupils over 12 and under 14	50.7	51.
Pupils over 14	51.	44.
EIGHT-PLACE NUMBERS.		
Pupils under 14 years	82.4	64.7
Pupils over 14 years	65.4	65.8

From this table it will be seen that in a majority of classes the girls make a decidedly less error than the boys. In the classes where the boys surpass the girls, it is by a very small figure. This conclusion harmonizes with the results of other observers.[1]

Unconscious Memory and Effect of Fatigue.—The tests were taken in three different Grammar Schools: Oxford, Freeland and Woodland Street Schools. In the Oxford Street School the four tests were taken in the morning and a

[1] A Statistical Study of Memory and Association, by Prof. Joseph Jastrow, Educational Review, Dec., 1891.

different series of digit-groups were used at each test. The same digits dictated at the first test were read in the inverse order at the second. They were then completely re-arranged for the third and read in the inverse order for the fourth. Thus the digits in every observation were the same for the four tests, the order alone being changed. This same arrangement was used in the tests of the Freeland Street School, two tests being taken in the morning immediately after the school assembled, and two just before closing in the afternoon. In the Woodland Street School the same digit-groups were used for all four tests, the purpose being to determine the effect of unconscious memory.

In the following Table the probabilities of correct judgments for each test on five, six, seven and eight-place numbers for all the pupils in Oxford Street School are given.

TABLE V.—Shows the probabilities of correct judgments in the Oxford Street School for the four tests with five, six, seven and eight-place numbers; 136 pupils were tested in this school.

No. Digits.	First Test. A. M.	Second Test. A. M.	Third Test. A. M.	Fourth Test. A. M.
5	82.7	91.3	83.	91.3
6	53.1	73.	71.	73.5
7	30.4	30.1	33.5	39.2
8	17.6	16.3	19.4	25.

In this school, where different series of digit-groups were used at each test, the pupils show with two exceptions considerable though not uniform increase in their ability to remember the groups of digits. This increase may be fairly taken to be the effect of practice, as the pupils remained naïve as far as possible with respect of the tests that were to be used.

TABLE VI.—Shows the probabilities of correct judgments for the Freeland Street School on four, five, six, seven and eight-place numbers. The digit groups that were used in the Oxford Street School were used in this school. Two tests were made in the morning and two in the afternoon; 219 pupils were tested in this school.

No. Digits.	First Test. P. M.	Second Test. A. M.	Third Test. P. M.	Fourth Test. A. M.
4	92.	95.	98.	95.
5	79.3	86.7	95.9	79.5
6	60.1	65.6	64.7	60.2
7	37.9	43.2	43.3	44.6
8	25.6	25.7	32.7	32.2

This Table shows that the pupils improved considerably though not uniformly with each test. They do not show greater increases for the morning than for the afternoon tests as we should expect from the fatigue of the day's work.

TABLE VII.—Showing the probabilities of correct judgments for the Woodland Street School on four, five, six, seven and eight-place numbers. The same series of digit-groups were used in all four tests. Two tests were made in the morning and two in the afternoon; 468 pupils were tested in this school.

No. Digits.	First Test. A. M.	Second Test. P. M.	Third Test. A. M.	Fourth Test. P. M.
4	96.2	97.3	98.	97.7
5	88.6	92.	94.2	94.3
6	56.7	64.4	70.1	75.5
7	40.4	50.7	58.7	64.1
8	28.4	34.9	45.9	49.7

In this school the pupils have shown uniform improvement in each test and at the same time the percentages of increase are usually larger. The morning tests do not show greater proportional increases than the afternoon.

The results from all the schools point to the conclusion that the pupils improve with practice. The great uniformity and large increases with each test in the Woodland Street School seem to show that the pupils unconsciously remember the digits that have been dictated one day previous. The probabilities of correct judgment do not show any variations due to fatigue. The total number of correct judgments for the morning tests in the Freeland Street School are 2,69 and for afternoon tests 2,640; for the morning tests in Woodland Street School 6,609, and for the afternoon tests 7,179. When we consider that great increases were made with each test, and the first test in the Freeland Street School was made in the afternoon, we should expect a greater number of correct judgments for the morning test; and since the first test was made in the Woodland Street School in the morning, we should expect a greater number of correct judgments for the afternoon test. This is just what the figures show, and we may safely conclude that the pupils suffer no fatigue from their school work, at least none discoverable by such tests as these. Their work is probably not excessive.

The Nature of Errors.—A careful examination of the observations shows that there were three, perhaps four, classes of errors which represent stages in the fading of the memory-image. In the first stage the digits suffer a displacement of order; in the second, other digits are substituted for some that were dictated and in the third, the number of digits is misjudged, either over- or under-estimated. Various causes may be assigned for the displacement of order. When the pupil attempts to write, the attention passes over the successive digits in memory as a rule much faster than they can be written. Before the pupils can write the first digit, the attention has passed to the third or fourth and the hand is innervated for the digit that is present in consciousness. The second may be immediately recalled and is put in the third place. It more frequently happens that the fourth or fifth is displaced than the second or third. Again, the order of the digits in the numbers previously dictated clings in the mind and causes the figures in the next number to be interchanged in accordance with that order.[1] A single case will be sufficient to make this statement clear. Two numbers, the first commencing with 8163 and the second with 5136, were dictated. The 3 and 6 in the second were frequently reversed so as to read 63. Further, the order in which the digits stand in our system of notation

[1] Dr. Leo Bergerstein, Zeitsch. f. Schulgesundheitspflege, No. IX and X. 1891.

determines some changes. One case in particular deserves mention and will be of service to others who attempt any work in this line. The last three digits of one number were 768 and a very frequent error was to change the order of the 7 and 6 to 67. It is probable also that when these digits were read in the inverse order, 867, the order of the 6 and 7 is again changed to read 876. In other cases a digit seems to have become associated with one place in the number from having frequently occupied that place, and when this digit appears in the next succeeding number, it changes places with the one that occupies the position it has become associated with; even when it does not appear in the following number, it may be substituted for one that occupies its associated position. In many cases it is very difficult to determine what has brought about the change and whether the error is an error of inversion or substitution. Number habits and the association of one digit with another from some experience in life—the number of the house, the year or day of the month of a child's birth—would seem to enter as factors. A fact that has been noticed frequently in teaching children and also adults is the great liability to confusion, when it is attempted to keep separate two like organs whose functions are diametrically opposed. In physiology it is a difficult matter for children to distinguish the functions of the right and left ventricles of the heart and even for adults the functions of the dorsal and ventral columns of the spinal cord. It seems probable that this difficulty also appears in keeping the order of two digits that are easily remembered. 1 and 5 standing at the end of a number, where the digits are rarely forgotten were frequently interchanged. The inversion of the order is by far the most frequent error, as it is also the first to occur.

In substituting a new digit for one that has been read, there enter some of the causes that bring about an inversion of the order. A digit is substituted for another to make the two stand in the order they do in our system of notation, or in the order in which they were in the number previously dictated. The likeness in the sounds of the names of two digits often determines the substitution in the one for the other. Nine and five and nine and one are frequently interchanged. The written or printed forms of 9 and 1 probably have something to do with the substitution of the one for the other. The very frequent interchange of 3 for 8 is due unquestionably to the likeness in the form of the printed digits. The likeness in the innervation required for two digits would seem to explain the substitution of 5 for 3 and 7 for 9. Substitution stands next in frequency to inversion of the order.

When the digits are left out the pupils more frequently have forgotten the proper digits and also their associations and so drop them out altogether. Whatever may be the cause of the dropping of a digit, the fact that it is left out shows a more advanced stage in the disappearance of the memory-image. The places in which the most errors of every kind are likely to occur are the positions from which digits are most frequently dropped. In some cases it seems probable that a digit may be dropped from the tendency to bring two associated digits in juxtaposition or two digits that stand juxtaposed either in our system of notation or in some number previously dictated.

When the pupils overestimate the number of digits, two tendencies only were noticed. The digits that were supplied were put in the places in our system of notation that occur between some two digits already given, or they were placed between two digits which should stand together and which were separated by the supplied digits in some number previously dictated. When two digits already stand in their natural order, the tendency is very strong to put another digit in order either before or after those given. The second tendency was to repeat some digit already written. Over-estimations of the number are very infrequent, probably for the reason that each test was begun with numbers that could be easily grasped and digits that could be counted. In the Normal School the observations were begun with seven-place numbers; but instead of making three observations, as was done in the Grammar Schools, with seven digits, the teacher dictated only one seven-place number before dictating an eight- and a nine-place number. Again, a seven- and an eight- and a nine-place number were dictated and so on until fifteen observations were made at each test. As the curves have shown that six figures are all that the best pupils can easily span, the Normal School pupils were taxed to the limit of their powers on the first trial. In the Grammar Schools the pupils were started with numbers they could easily grasp and were led by steps to expect the number of digits in each succeeding observation. On this account 180 pupils from the Oxford Street School over-estimated the number of digits on observations with seven- and eight-place numbers 88 times; and 24 Normal School pupils over-estimated the number of digits 76 times. In counting the errors that arise from dropping digits no separate account was taken of the cases where the pupils dropped the digits because they did not remember the number of digits given, and where they dropped them because they failed to recall the correct digits. In most cases it would be difficult to determine this. This

tendency, however, to over-estimate shown by the Normal School pupils is not the general rule, as the experiments of other observers have pretty conclusively shown. Drs. Hall and Jastrow[1] found that the tendency was to under-estimate the number of clicks made by a quill held against the notched circumference of a revolving wheel, when the number of clicks was too rapid to be counted. Other experiments point in the same direction. It would seem then that at the moment when pupils reached the limit of their memory span, they over-estimated the number of digits ; but if the experiments had been continued with a greater number still, the pupils would have under-estimated the number.

Method of Correcting the Observations and Counting the Errors.—The method of correcting and counting the errors has an important bearing upon the number and the classes and so must be given in some detail. Any treatment of the errors will necessarily be more or less arbitrary ; but that treatment which gives the most uniform results and can be the most consistently applied would seem to be the least arbitrary. Some account too must be taken of the number of errors. The least possible number of changes necessary to make a given number correct was made, and each change was counted an error and classed an error according to its kind. The numbers were corrected as far as possible by restoring the order, before resorting to the substitution of other digits. The digits not required were then cut out and the proper substitutions made, the last process being to supply the vacant places with digits. It would be possible, however, to correct any number by either cutting out digits or substituting others. This would reduce the number of classes of errors but would greatly increase their number and destroy the distinction between the classes, which is based upon three different psychological processes. After careful consideration this method seemed to give the most uniform results and was the easiest to apply consistently.

The Frequency of the Different Classes of Errors.—To determine the class of errors which was first to occur, all those observations in which only one kind of error was made, were examined on 105 tests from the fourth and fifth grades.

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[1] Studies of Rhythm, by Professor G. Stanley Hall and Joseph Jastrow. Mind, Volume XI., No. 41.

TABLE VIII.—Showing the different classes of errors.

	Inversions of Order.	Substitutions.	Over Estimations.	Under Estimations.
Five-place Numbers . .	10	7	3	1
Six-place Numbers . .	44	15	15	5
Seven-place Numbers .	40	26	13	2
Eight-place Numbers .	33	10	14	4

If we take observations with a much greater number of digits, we should expect that the greatest number of errors would result from the dropping of digits. This matter will be discussed again further on. The total numbers of errors of every class in the Oxford Street School, when compared with the total number of observations, give the following percentages: Inversions of the order, 53%; substitutions, 31%; under and over estimations of the number of digits 15%.[1]

Position of the Error.—In what places are errors most likely to occur, or in what places are the digits most often forgotten? We encounter here a new difficulty in deciding which digits are incorrect. This difficulty applies only to digits that are interchanged. Then, too, when the pupils have left out figures they frequently indicated them by leaving a blank space. The first three digits are frequently present in their proper order, then follows a blank space and the last two in their order. The last three may be present and the rest absent or *vice versa*. To avoid this difficulty, all the proper digits that stood in their proper order either from the beginning or the end of the number, were considered correct and all others incorrect. 300 observations from the

[1]Important in this connection are the experiments of Dr. H. Münsterberg, (*Die Association successiver Vorstellungen*, Zeitsch. f. Psychol. Bd. I., H. 2, 1890.) He found that he could repeat seven letters without error, when they had been exposed one at a time in such a way that each was seen for one second; but he reached the limit of his power at ten. The most frequent error was that of substitution; inversions of the order were very rare. When, however, he solved problems in mental arithmetic aloud, while the letters were being exposed, his upper limit fell to seven, and only four or five could generally be recalled correctly. Instead of errors of substitution simply, errors of inversion of the order became the more frequent. The explanation he offers for this does not seem in the light of these tests to be conclusive. Possibly the difference in rate at which numbers and letters were given may help to account for the difference.

senior class in the High School on six, seven, eight and nine-place numbers and 300 from the eighth and ninth grades of Freeland and Woodland Street Schools have been examined with reference to the place in which the errors were most likely to occur. The errors occurring in each place were counted and compared with the possible numbers. The percentages were marked upon six ordinates for six-place numbers, on seven for seven-place numbers and so on, and these points connected by lines. The curves thus obtained will represent the relative frequency with which an error occurs in each place. Chart III. shows the curves for the Grammar School grades and for the High School. Those for the Grammar School are represented by the unbroken line and those for the High School by the dotted line. All the curves show a gradual though not uniform rise from the first place to one place past the middle; the curves then fall at first slowly, then rapidly, until they reach a point in the last place almost as low as at the beginning, in two cases lower. If observations with more than nine digits were made, we should expect that the pupils would be likely to forget the first more often than the last, so that it is not a mere accident that the curve for nine-place numbers ends lower than it begins. If fifteen or more digits were made the subject of observation, the pupils would probably forget all but the last two or three with only an occasional recollection of the fourth from the last and the first. There is a backward flow to the memory from the last, which affects quite perceptibly the second, slightly the third and in individual cases the fourth from the end. (We have here a demonstration of the well-known rhetorical principle that the emphatic words in a sentence are the first and the last.) In addition to this we get an idea of the relative importance of the other words. This will be true of any series of successive ideas. They are permanent in an inverse order as they are removed from the beginning except the last two or three which are permanent in their order from the last.

Conclusions.—I. The limit to the memory span for the pupils in the public school is six.

II. The memory-span increases with age rather than with the growth of intelligence. Experience in this matter is a better school than books.

III. The memory-span measures the power of concentrated and prolonged attention.

IV. Intellectual acuteness, while more often accompanied by a good memory-span and great power of concentrated and prolonged attention, is not necessarily accompanied by them.

V. The girls have better memories than the boys.

VI. With practice, pupils increase in their ability to remember groups of digits.

VII. Pupils unconsciously remember digits that they heard a day before, when they are used second time.

VIII. The tests do not show that the pupils suffer fatigue from the day's work. This fact shows that the work in the schools is probably not excessive.

IX. Memory images pass through three stages in leaving the mind. First, they suffer a confusion of order; second, a loss of certain elements and the substitution of associated elements; and third, a complete loss of some elements and no recovery.

X. Ideas previously in the mind and association forms of ideas are factors in causing the confusion of the memory image and its final loss.

XI. There is an apparent tendency to over-estimate the number of ideas presented to the mind, when the number of ideas is slightly greater than the memory span; but the general rule is to under-estimate the number.

XII. Ideas, except the last two or three in a series, are lasting in an inverse order as they are removed from the beginning of the series in which they occur. The last two or three are lasting but in decreasing degree as they are removed from the end of the series.

STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.—II.

BY JOSEPH JASTROW, PH. D.

A STUDY OF ZÖLLNER'S FIGURES AND OTHER RELATED ILLUSIONS.

(With the assistance of HELEN WEST.)

The present paper describes an investigation of an illusion which, while familiar and frequently studied, remains in its essence and conditions of origin quite unexplained. We make no claim of furnishing an adequate and final explanation, but simply aim to establish a few steps in that direction. The illusion is that so well marked in figure 1, first described by Zöllner[1]. In this figure the main lines appear very far from parallel; each adjoining pair of lines seems to converge at one end and diverge at the other. Here we have a com-

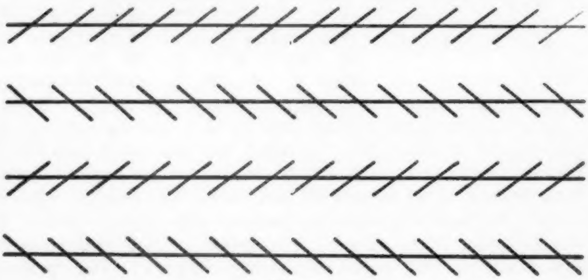


FIG. 1.

plex form of the illusion involved, and it was our problem to ascertain the preceding members of the series of

[1] We are indebted for the use of figures 10, 11, 12, 13 and 24 to the courtesy of Messrs. Charles Scribner's Sons, publishers of Ladd, Outlines of Physiological Psychology, and for figures 1, 14, 15, 17, 18 and 25 to Messrs. Henry Holt & Co., publishers of James's Psychology, which courtesies we acknowledge with gratitude.

which this is the end term. It would be tedious to describe the various steps by which we stripped this figure of one and another of its complications, determining in a variety of ways what part they played in the total effect; it will be more acceptable to substitute for this rather laborious process an exposition beginning with the simplest type of the underlying illusion, and building it up step by step to its most complicated form.

When viewing two lines separated by a space, we are able to connect the two mentally and determine whether they are or are not continuations of one another; but if we add to one of the lines another meeting it so as to form an angle, the lines which seemed continuous no longer appear so, and those

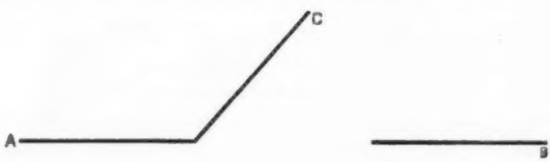


Fig. 2.

which were not continuous may appear so. In Fig. 2 the continuation of the line A appears to fall below the line B, and similarly the continuation of C apparently falls to the right of D. But in reality A is continuous with B, and C with D. If we cover the line C, A and B seem continuous; thus indicating that the illusion is due to the angle. What is true of obtuse angles is true, though to a less extent, of right angles and of acute angles ; in brief, the degree of this illusion of discontinuity increases and decreases as the angle increases and decreases. The figures to prove this the reader can easily supply ; further illustration thereof will appear later. This is the simplest form of a sense deception that underlies very many familiar but more elaborate figures. The principle therein involved we generalize as follows : Calling the direction of an angle, the direction of the line that bisects it and is pointed toward the apex, then *the direction of the sides of an angle will be deviated toward the direction of the angle*. A very important corollary of this main generalization emphasizes the point, that just as the deviation of direction is greater with obtuse than with acute angles, so also when

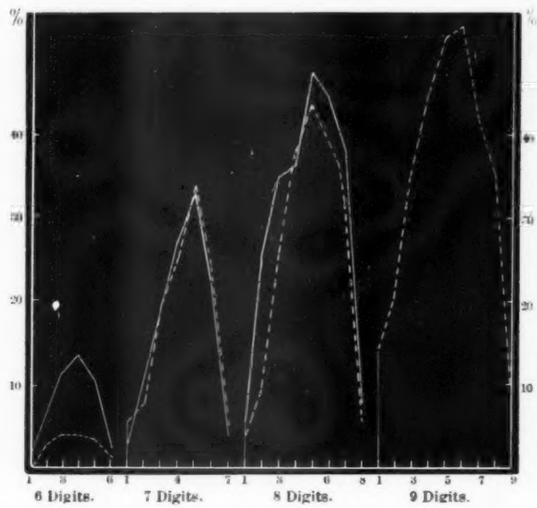


CHART III.—Shows the relative liability of a digit to be forgotten in any order in six, seven, eight and nine-place numbers. The unbroken line represents the distribution of errors for eighth and ninth grade pupils, and the broken line the distribution for High School pupils.

obtuse and acute angles are so placed as to lead to opposite kinds of deviation, the former will out-weigh the latter, and the illusion will appear according to the direction of the obtuse angle.

We proceed to notice a few of the means by which the illusion may be varied and tested. A relatively large distance between the lines, the continuity of which is to be judged, produces a more marked illusion than a relatively small distance. The appropriate figures the reader can readily supply. In other words, opportunity must be given for the eye to lose itself in passing from the one line to the other. The degree of illusion may be increased by increasing the number of angles in various ways. We may draw a series of oblique lines parallel to the line C (in Fig. 2) and joining the

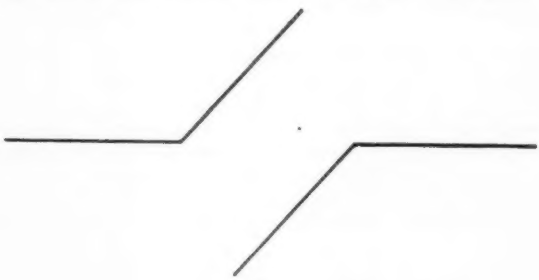
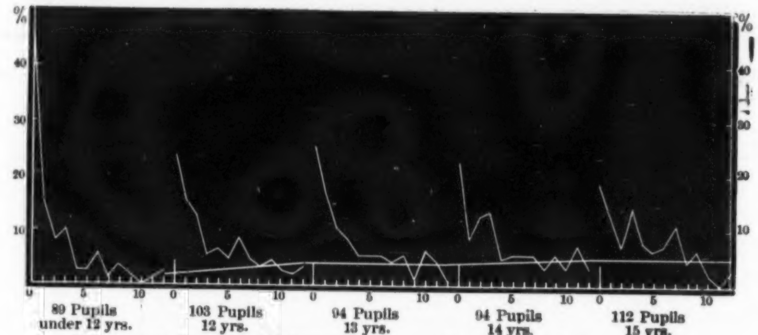


FIG. 3.

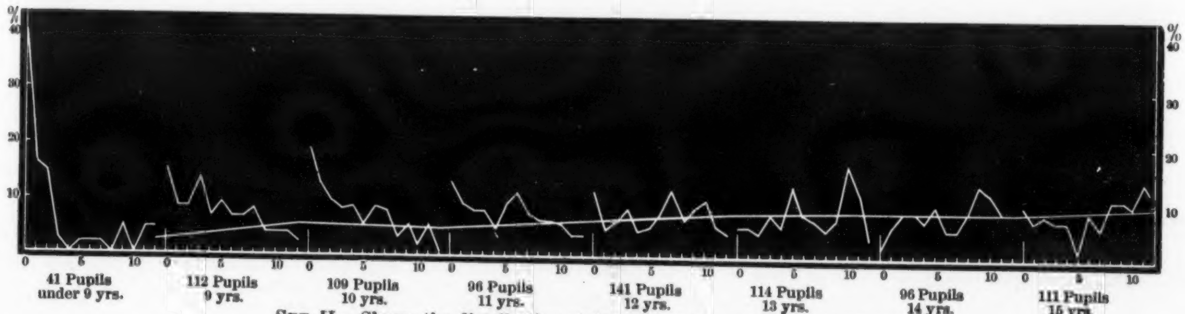
line A. Or again we may draw a line parallel to C from the left-hand end of line B. This gives Fig. 3, in which the two horizontal lines seem to be on entirely different planes. The direction of the deviations induced by these angles being opposite in tendency, the result is quite marked. Again we may add a second line parallel to the real continuation, which will be the apparent continuation, and we may further strengthen the tendency to regard the non-continuous line as the true continuation by shading them alike or otherwise differentiating them. Again, we may draw this second line slightly

FIG. 4.

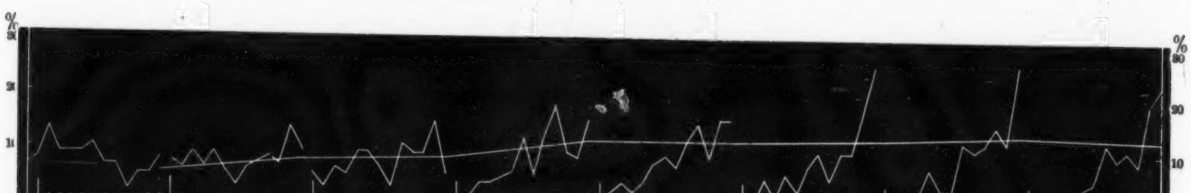
oblique instead of parallel with the first line with good success; this is done in Fig. 4. Both of these tests can be made

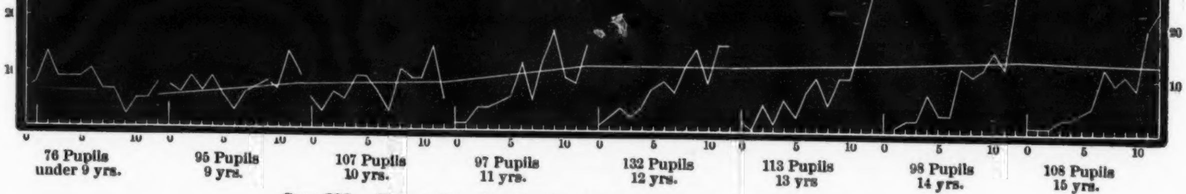


SET I.—Shows the distribution of the pupils on eight-place numbers.

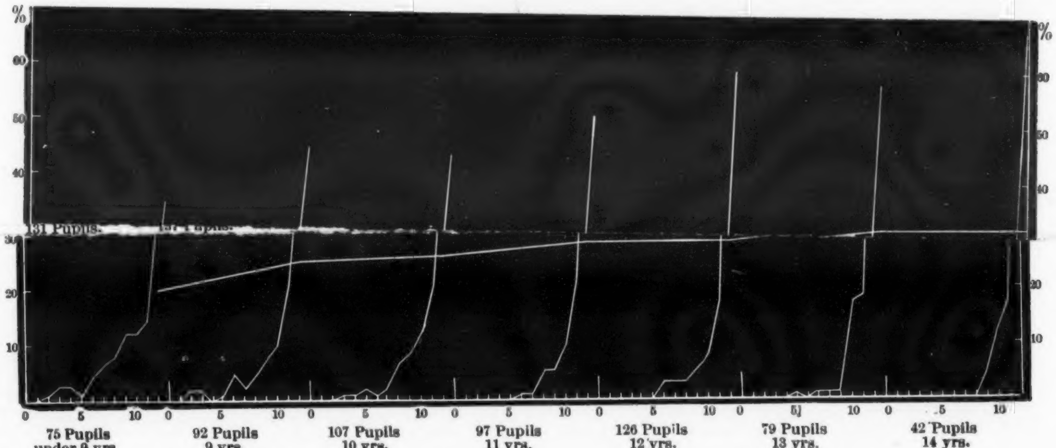


SET II.—Shows the distribution of the pupils on seven-place numbers.



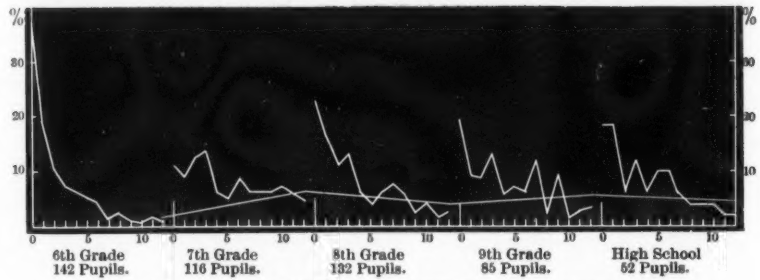


SET III.—Shows the distribution of the pupils on six-place numbers.

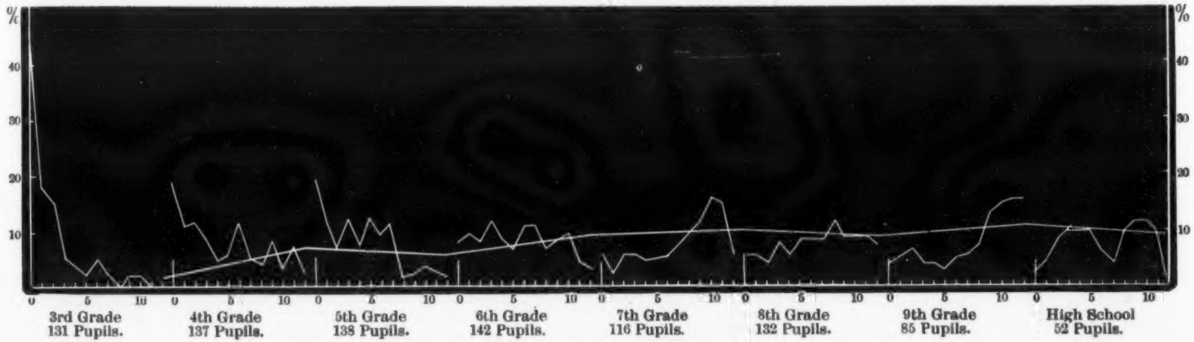


SET IV.—Shows the distribution of the pupils on five-place numbers.

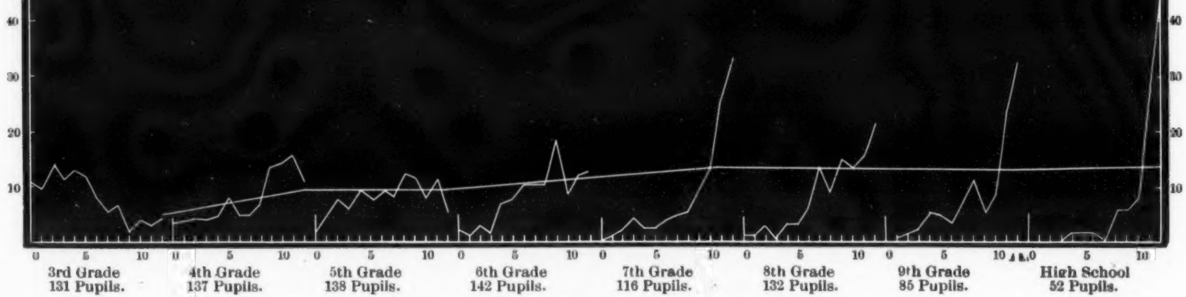
CHART I.—Shows the classification of the pupils according to age.



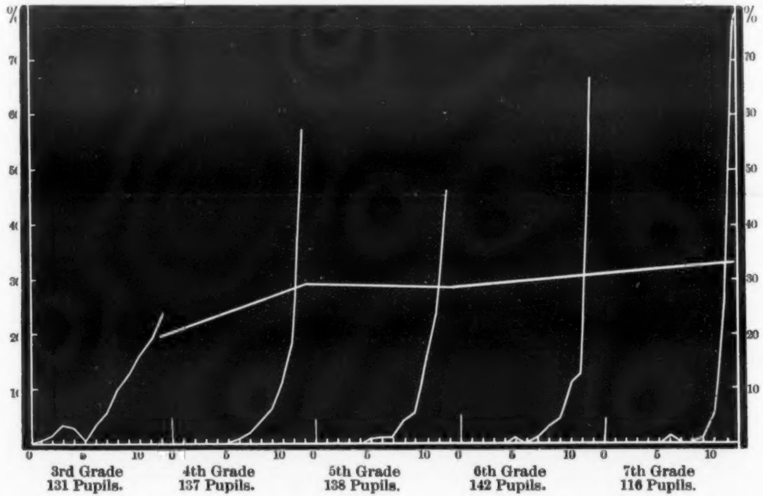
SET I.—Shows the distribution of the pupils on eight-place numbers.



SET II.—Shows the distribution of the pupils on seven-place numbers.



SET III.—Shows the distribution of the pupils on six-place numbers.



SET IV.—Shows the distribution of pupils on five-place numbers.

CHART II.—Shows the classification of the pupils according to grade.

accurate by measuring the maximum deviation between the parallels or between the parallel and the adjacent oblique line, which the eye will tolerate and still retain the illusion of the false continuation; or again the angle alone might be drawn and the error measured, which the subject would make in adding what appears to him a true continuation of the sides.

On the basis of the general principle above enunciated, we may proceed to the explanation of a series of more complex figures. We turn to Fig. 5. Here the effect of the obtuse

FIG. 5.

angle ACD is to make the continuation of the line AB fall below the line FG, while the effect of the acute angle is just the reverse, but, by our corollary, the former preponderates over the latter and directs the illusion. The line EC adds nothing essential to the figure, for it simply introduces two angles, ECB and ACE, which reinforce the angles ACD and BCD. Likewise the line BC might be omitted or covered, and leave the illusion essentially unaltered. In Fig. 6 we observe a slightly

FIG. 6.

different form of the illusion, the continuation of each line appearing to run below that of the other, so that these continuations would meet at an obtuse angle. All these variations follow from the dictum that the direction of the side of an angle is deviated toward the direction of the angle.

We may further note those cases, in which the effect of each angle is counteracted by that of another, resulting in the disappearance of all illusion; this occurs when all the angles are equal, that is, are right angles. This appears in Fig. 7 and would appear equally well in any form of a rectangular cross with lines continuous with any of its arms. If we omit or cover the portions of the vertical lines below the horizontal in Fig. 7, we obtain a very instructive figure. If we observe the horizontal lines, we notice that they do not appear perfectly horizontal, but each appears to tip upwards slightly from the apex; i. e., is deviated toward the direction of the angle; so

also if we observe the vertical lines, we notice that they do not appear exactly vertical and parallel, but the right hand

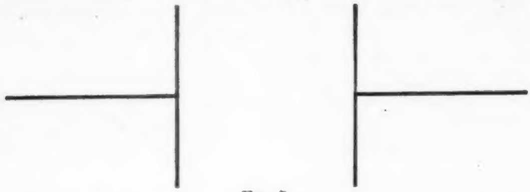


FIG. 7.

line tips slightly toward the right, the left hand line toward the left; i. e., they are likewise deviated toward the directions of their angles. This tendency of the sides of an angle to be deviated toward the direction of the angle, may result not only

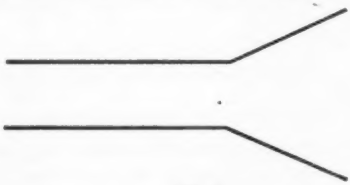


FIG. 8.

in making continuous lines appear discontinuous, but also in making parallel lines appear to diverge from parallelism.

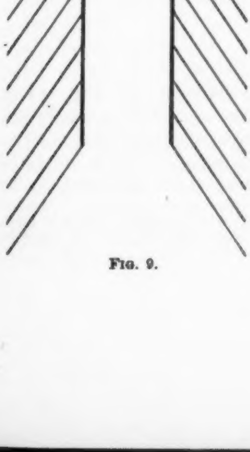


FIG. 9.

We may further illustrate the relation of divergence from continuity to divergence from parallelism by rotating the right half of Fig. 3 through 180°, and placing it under the left half. In this way we obtain Fig. 8, which shows that the same angles as readily produce slight deviation from parallelism as from continuity. To strengthen this illusion, we multiply the number of oblique lines and thus of obtuse angles. In so doing, we unavoidably introduce acute angles, but as before their effect is out-weighted by that of the obtuse angles. We thus obtain Fig. 9, in which the parallel lines diverge markedly above

and converge below. If we now carry the diagonal lines *across* the vertical ones, the illusion remains, and it is clear from our dictum that it should (v. explanation of Fig. 5.) By simply adding more main lines, we have the figure of Zöllner, with which we set out[1].

Having thus given a resumé of the series of illusions from simple to complex, we may proceed to apply our principles to the explanation of other forms of the illusion. Fig. 10 shows the illusion of discontinuity; the line *a* appears continuous

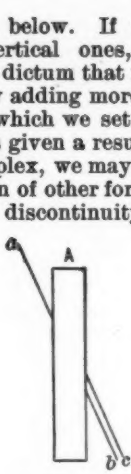


Fig. 10.

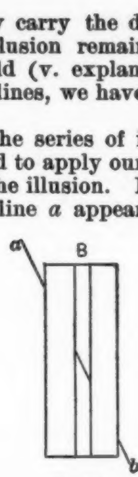


Fig. 11.

with *c*, but is so with *b*; and this is neatly emphasized in Fig. 11, in which a continuous line is deviated once in one direction and again in the opposite; the use of rectangles, instead of pairs of vertical lines, makes no essential difference. Fig. 12 presents the same illusion with the lines horizontal, the line *a* appearing continuous with *c*, while it is so with *b*. In each case the obtuse angle out-weighs the acute angle and determines the direction of the deviation. Fig. 12, when contrasted

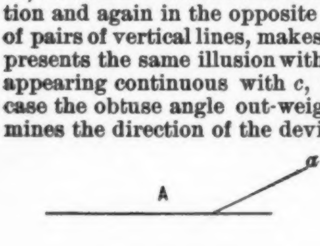


Fig. 12.

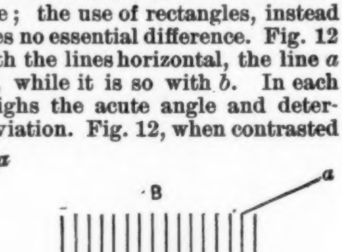


Fig. 13.

with Fig. 13, shows the effect of the position of its angles; in the former, *c* seems continuous with *a*, while *b* is really so, because the lower obtuse angle attracts the deviation of the line *c* towards itself; in the latter, the obtuse angle actually drawn between *c* and one of the vertical lines out-weighs in

[1] The oblique lines have been made shorter, but this does not add anything essentially different. It keeps the figure compact, and thus readily allows the judgment of parallelism.

effect the angle that is suggested between c and the horizontal line formed by the end-joints of the vertical lines, and thus the true continuation of a is below the apparent continuation. The divergence and convergence of the horizontal lines in Figs. 14 and 15 likewise follow from the above principles and

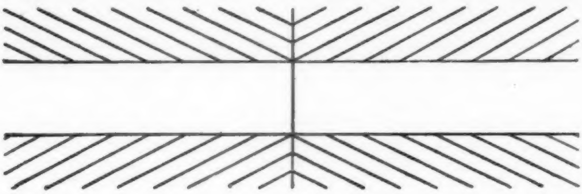


FIG. 14.

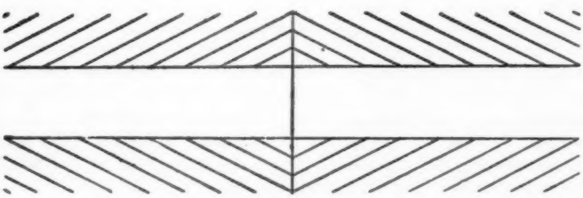


FIG. 15.

illustrate some of its more complex forms, while most complex and brilliant of all is Fig. 16. In these figures the same

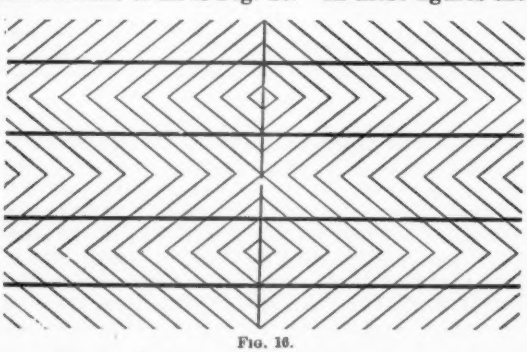


FIG. 16.

line is made to deviate in opposite directions (by oppositely directed obtuse angles) from its centre, and thus theconverg-

ence and divergence of the lines is greatly emphasized. The points at which the apparent change of direction occurs are also emphasized by cross lines. Fig. 17 adds the further

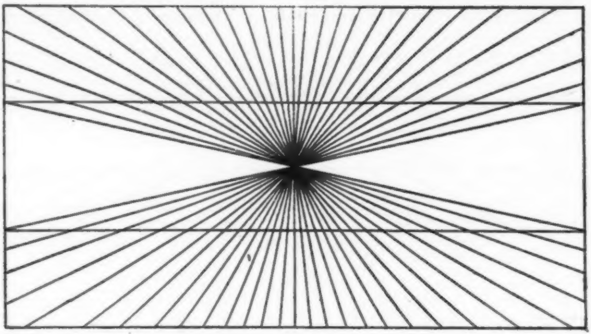


FIG. 17.

principle that the extent of the apparent deviation varies directly with the size of the angle, for as each successive angle increases (or decreases), the deviation increases (or decreases), so that the straight line becomes a line with a continuous change of direction, that is, a curve; as before, the obtuse angles are the significant ones.

Helmholtz finds a similar illusion in which motion is involved and which Prof. James thus describes (Fig. 18.)

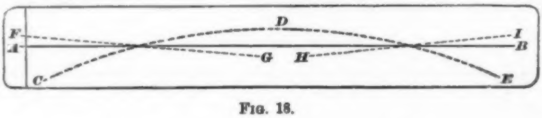


FIG. 18.

"Let A B be a line drawn on paper, C D E the tracing made over this line by the point of a compass steadily followed by the eye as it moves. As the compass point passes from C to D, the line appears to move downward; as it passes from D to E, the line appears to move upward; at the same time the whole line seems to incline itself in the direction F G during the first half of the compass's movement, and in the direction H I during the last half; the change from one inclination to another being quite distinct as the compass point passes over D." The line formed by the movement of the compass points acts as two oblique lines crossing the horizontal one. Curved lines produce the same illusion, as may be

seen in Fig. 19, by the apparent sagging of the lines at the centre. The illusion is here strengthened by the presence of several curves.[1]

FIG. 19.

[1] Wundt figures two illusions, which, apparently, are exceptions to our generalization, and which, accordingly, demand attention. In Fig. 20 the horizontal line appears as two lines tipping slightly downward

FIG. 20.

from the centre. Our first impulse would be to regard the illusion as due to the angles *ACE* and *BCF*, and we should, according to our dictum, expect the lines to tilt *upward* slightly. But remembering the greater effect of obtuse angles, we should view the figure as composed of Fig. 6, in which the two horizontal lines are approached to one another until they meet; when, by the effect of the angle *ACF*, *EC* is tipped down, and by the action of *BCE*, *CF* is tipped down. That such is really the natural way of looking at it will be evident from Fig. 14; at the centre of the upper line we have the very same arrangement of lines producing the same effect, and immediately in conjunction with the effective obtuse angles.

Wundt's next figure is more difficult to explain (Fig. 21). There the

FIG. 21.

lines tip up at the ends, and there are apparently no angles which would make them do so. We have come to the view that this figure is a modification of Fig. 22. Here the obtuse angles are present and determine the

FIG. 22.

illusion. The oblique lines need not join to produce the effect, and the short vertical line, as in the other cases, simply brings out the point at which its change of direction takes place. We judge the tipping of the lines by reference to a horizontal which we carry with us or have suggested by the line of the page or the many horizontal objects we behold. We must likewise infer that the tipping in Fig. 21 is due to the obtuse angle formed by one side really present, and another suggested only by the continuation of *CE* and *CF*. The following considerations may serve to remove the artificiality of this explanation: (1) we do frequently judge by reference to imagined lines; e. g. our horizontal and vertical; (2) we use suggested lines in illusions; (3) the centre of Fig. 5 above presents Fig. 21 as the end term of a series, and in conjunction with the effective obtuse angle; (4) the illusion increases as this imagined obtuse angle increases, but decreases as the real acute angle increases.

The last illusion to which we shall attempt to apply our dictum is the familiar one whereby a square enclosed within a circle seems to bend in the circle at the four points of contact. That the circle contributes no essential part to the illusion can be shown in Fig. 23, in which the rectangle in-

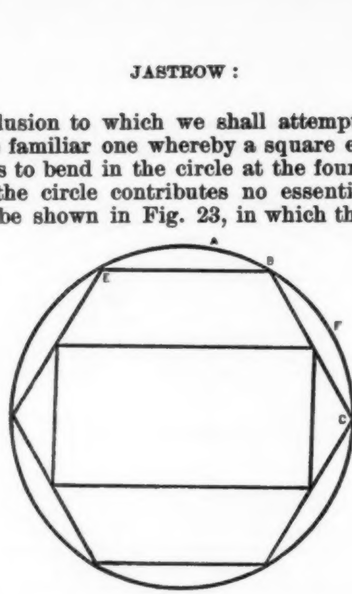


FIG. 23.

scribed in the hexagon bends in the sides of the hexagon at the points of contact. Furthermore, the fact that the hexagon shows the illusion even better than the square, suggests that the larger angle is the more effective. This illusion we regard as a complicated form of Fig. 20 inverted. From our former explanation it is already clear why we should have a large obtuse angle at the point of contact instead of the actual continuous line. The portion of the curve adjoining the point of contact acts as a straight line, as it also does in Fig. 19. The effective angles are thus the angles of which *ABC* and *DBF* are types. The application of our generalization to forms of illusion not taken into account in the formulation is a very gratifying index of its value.

We turn finally to a brief account of the literature of the topic. Zöllner accidentally noticed the illusion on a pattern designed for a print for dress goods. He established the following points: (1) the illusion is greatest when the main parallel lines are inclined 45° to the horizontal; (2) the illusion disappears when viewed at a slight angle, as by holding one corner of the figure up to the eye; (3) it vanishes when held too far from the eyes to clearly see the cross striations; (4) the illusion is as good for one eye as for two; (5) the

strength of the illusion varies with the inclination of the oblique to the vertical lines. In later studies he determined that (6) the angle between oblique and vertical lines at which the illusion is greatest is 30 degrees; (7) the illusion appears under the illumination of an electric spark quite as strongly as otherwise; (8) viewing it through red glass weakens it.

He also answers criticisms by Helmholtz and Hering. His explanation is curious and in its details unintelligible. He draws an analogy between these and illusions of motion and makes all depend on the view that it takes less time and is easier to infer divergence or convergence than parallelism.[1] Why the illusion should vary with the angle, under this theory, he does not explain; the fact that it is greatest at 45° he regards as the result of less visual experience in oblique directions. Apart from the fact that this theory does not explain and is not applicable to many of the figures, it can be experimentally disproved by a figure similar to Fig. 1 but with the lines actually inclined but apparently parallel, as suggested by Hering. Here really divergent lines all seem parallel, showing that the illusion does not consist of the inference of parallelism or non-parallelism, but of a certain angular distortion of the real relations of lines.

Hering (*Beiträge zur Physiologie*, 1861, pp. 69—80) added several of the figures above noticed (Figs. 14, 15, 19). He bases his explanation upon the curvature of the retina and the resulting difference in the retinal images of arcs and circles. He figures this explanation for the square enclosed in a circle and applies it to the rest. He criticises Zöllner and dismisses the fact that the illusion is strongest in oblique directions, as irrelevant. In a later article (Hermann, III., p. 373), he brings in the additional statement that acute angles appear relatively too large and obtuse ones too small.

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[1] It is well to note that Poggendorf called Zöllner's attention to a further illusion in his figure. This was printed in deep black lines, and the two parts of the oblique line crossing it seemed not quite continuous; *i. e.*, the illusion of Fig. 10, with a broad black line for the rectangle. Zöllner regarded this as unrelated to the other and accredited it to astigmatism.

He also prints a figure (Fig. 24) based upon the fact of greater illusion in oblique directions. This figure, as Aubert has pointed out clearly, refutes Hering's theory, for it shows

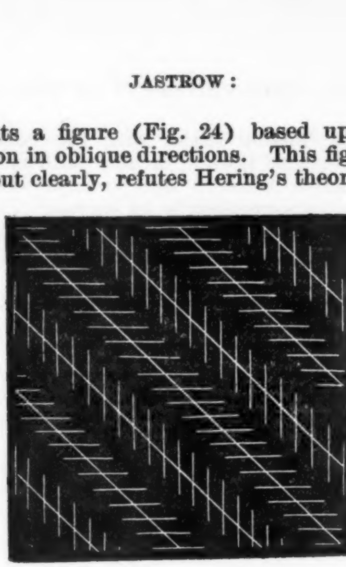

FIG. 24.

a variation in the strength of the illusion, while the retinal image remains the same.[1]

Aubert *(Physiologie der Netzhaut,* 1865, pp. 270—272) confines his attention to a notice of the results and views of others, closing with the sentence: "I am unable to give any explanation of Zöllner's illusion." In a later work, *(Physiologische Optik,* 1876, pp. 629—631), he practically repeats his former statements, and mentions that Volkmann explained it by an apparent alteration of the plane in which the oblique lines appeared; *i. e.*, they appeared in a plane inclined to that of the paper, and the inclination of the long parallel lines to this plane appears as inclination toward one another.

Classen *(Physiologie des Gesichtssinns)* after disagreeing with all previous theories, gives his own explanation in these words: "Now the cause of the illusion is clear: in recognizing the directions of the converging and diverging oblique lines, we judge them by their relations to the vertical ones. These recede from the oblique lines where they diverge, and approach them where they converge; and thus the direction

[1] Kundt (cited by Aubert) attempted to get an experimental proof of Hering's view, but his results at close distances, which alone are relevant, failed to corroborate the theory. Kundt also determined the relation between the size of the figure and the distance from the eye at which the illusion disappeared.

of the verticals is regarded as a separation toward the side of convergence and an approaching toward the side of divergence." Classen noticed that the illusion appeared as soon as a pair of parallels was crossed by a pair of oblique lines which formed an acute angle at their junction. He insists that a pair of lines of opposite direction is necessary to produce the illusion, and leads one to infer that if this were not so his theory would be disproved. This can be shown in various ways; *e. g.*, by drawing only the left half of Fig. 9 and substituting a parallel line for the right half, the illusion remains, though not so distinctly.

Lipps (*Grundthatsachen des Seelenlebens*, 1883, pp. 526—530) regards the illusion as primarily psychical; whatever parts the movements of the eyes play being determined by the attention. He says: "If we draw (Fig. 25) the line *pm*

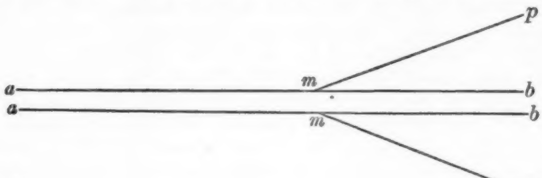

FIG. 25.

upon the line *ab*, and follow the latter with our eye, we shall, on reaching the point *m*, tend for a moment to slip off at and to follow *mp*, without distinctly realizing that we are not still on the main line. This makes us feel as if the remainder *mb* of the main line were but a little away from its original direction. The illusion is apparent in the shape of a seeming approach of the ends *bb* of the two main lines." Prof. James, whose words we have been quoting, adds: "This, to my mind, would be a more satisfactory explanation of this class of illusions than any of those given by previous authors, were it not again for what happens in the skin." Prof. James thinks that this class of illusions belongs to the field of sensation rather than of unconscious inference.

Hoppe (*Physiologische Optik*, 1881, pp. 73—83) gives a careful and critical digest of the views of others, which he finds it difficult to understand. His own is no less so but seems to be that our eyes and our attention are drawn to those lines that do most to fill out space, and that we run out the oblique lines until they meet; from this imagined point of junction the real parallels look divergent. When we carefully fixate the parallel lines, the illusion is avoided. Or

again, "we follow the *filled* middle space according to the course of the oblique lines and *neglect* the black parallel straight lines; or, because the latter are thus less noticed and viewed from a greater distance, we strengthen the appearance of their separation by the indirect view we obtain of them." The illusion is not retinal, because it vanishes in the after image; it is intellectual in origin. It is difficult to see how Lipps's view would be expressed so as to apply to Fig. 2, or why the illusion should disappear in Fig. 7. Hoppe's view is open to the same objection as Classen's and is refuted by the same figures.

Wundt *(Physiologische Psychologie*, 3d ed., II., pp. 124–132) although bringing in other factors as well, makes his main argument rest upon the view that we tend to overestimate acute and underestimate obtuse angles. He gives no proof of this fact, if fact it be, nor explains in what manner the error appears. He seems to mean that, in judging of the direction of the sides of an angle, we view acute angles as larger than they really are. If this be so, there must be some angle at which the illusion disappears, and this would seem to be the right angle; however, we get the illusion with right angles. Again, in Fig. 5 and many similar figures that we could construct, the acute angle judged by the same means would appear to be smaller than it really is, and in many respects acute and obtuse angles are affected alike. In common with others, Wundt regards the increase of the error at 45° as due to a less exact visual experience.

Pisco *(Licht und Farbe*, 2d ed., 1876, p. 268) gives no explanation, but adds the beautiful Fig. 16.

Helmholtz *(Physiolog. Optik*, pp. 564–574) presents a peculiar view of the subject. He begins with the illusion of the deviation in direction of the two parts of an oblique line separated by a rectangle and regards the particular cause of this illusion to be the curving in of the oblique lines as they meet the sides of the rectangle or heavy vertical lines. Moreover, this is especially true of small figures, in which as a whole the illusion is more marked. This deviation, then, at least in small figures, is due to irradiation. He supplements this explanation with one that will apply to large figures and to Zöllner's illusion. He says: "We may consider these illusions as new examples of the law above indicated, according to which acute angles, being small in size and clearly limited, appear in general as too large when compared with right or obtuse angles." Moreover, movement plays a large part in at least some of the figures, and in these the illusion disappears under precise fixation and the electric spark. This effect of movement is illustrated by the instance

cited above and leads to a sort of contrast whereby a clearer difference seems a larger one. Besides the general objection that so many principles are brought in to explain facts so clearly belonging to one sphere, and the further objections which have already been advanced against the alleged over-estimation of acute angles, several detailed criticisms might be made. In the first place, Helmholtz has not shown that small figures present the illusion better than large ones; in his figures he has drawn less than half as large an acute angle in the small figure as in the large one, and this is the cause of the difference he observes. Regarding the alleged curvature of the lines, it is difficult to see it; and it, as well as the possibility of irradiation, may be eliminated by drawing all the lines light and not allowing the oblique line to quite meet the vertical ones—under such circumstances the illusion persists. Helmholtz's chief argument for the effect of fixation is drawn from the heavily-drawn form of Zöllner's figure, in which he looks at the white bands with oblique lines running out like the feathers on an arrow, and *sees them parallel:* but this is precisely what *must occur* from the position of the angles, the effect of each angle being compensated by another. The two modes of drawing the figure make two figures of it. The arguments from the electric spark experiments are certainly questionable both in fact and inference, and it must be admitted that the entire treatment is unsatisfactory.

It will be seen that the field we have entered is a very complex one, and that a most important problem—Why do we deviate the sides of an angle toward the direction of an angle?—remains to be solved. How far does this depend on eye movements, how far upon inference, and the like? The chief defect of former attempted explanations seems to us to consist in theorizing upon too limited a range of facts. What is true of one group of figures fails to apply to others. Before an explanation can be satisfactory we must know precisely what it is that we are to explain, and this necessitates a correct and comprehensive generalization of the facts: this it is that we have attempted to supply.

Our study of these illusions leads us to regard them as essentially psychological in origin; they are illusions of judgment and not of sensation. Furthermore, we would regard them as an outcome of the general principle that we are prone to judge relatively rather than absolutely; that our perceptions differ according to their environment; that a sense impression is not the same when presented alone and when in connection with other related sense-impressions. A line presented

by itself is a different object from a line as a part of an angle or of a figure. However much we desire to consider the line independently of the angle, we are unable to do so. We have the direction of an angle and the direction of the lines that form the angle, and we are unable to consider the latter absolutely without reference to the former. The more nearly the directions of the angle and of the sides coincide, i. e. the smaller the angle, the smaller will be the error induced by this relative mode of viewing the lines. The whole series of illusions would thus be subsumed under the law of contrast or better of relativity; and the different variations and degrees of the illusion would find their explanations in the readiness with which they suggest and enforce misleading comparisons.

In order to exhibit a type of illusions most readily explicable from this point of view, as well as to exemplify the suggestiveness of the latter, we will consider an allied group of usual illusions.

Just as the presence of angles modifies our judgment of the *directions* of their sides, so too, the angles will modify the apparent *lengths* of lines. This form of contrast is most strikingly exhibited in Fig. 26, and best by comparing I and IV, i. e. cover up II and III. It seems almost incredible that the horizontal portions of I and IV are of equal length, and yet such is the case. II and III supply the intermediate steps, and in comparing the four figures the horizontal portions seem to become successively shorter from I to VI, while,

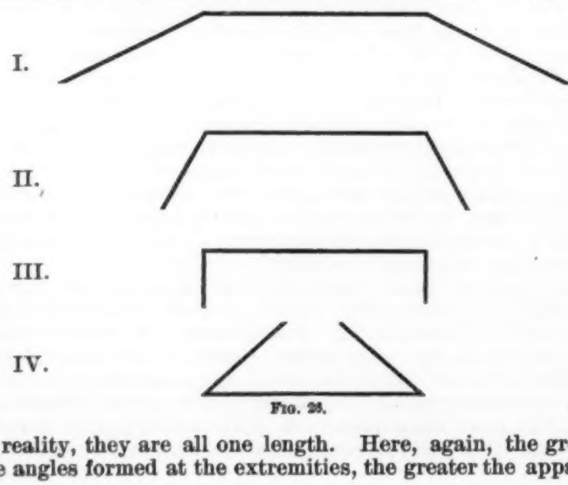

Fig. 26.

in reality, they are all one length. Here, again, the greater the angles formed at the extremities, the greater the apparent

length of the line; and thus the constrast is greatest between the very obtuse and the acute angles. Other factors contribute to the illusions; e. g. the positions of the figures, the juxtaposition of certain lines, the distance between the figures, and the like. The illusion persists if the horizontal lines be omitted, and we judge the spaces between the oblique lines. It also shows very well by cutting the figures out of paper either as they are or as truncated pyramids (by joining the ends of the oblique lines by a line parallel to the horizontal one), and viewing them against an appropriate back-ground.

We may also be tempted to judge of two areas by their juxtaposed lines, thus regarding one of two equal areas as larger than the other. This is shown in Fig. 27, which also

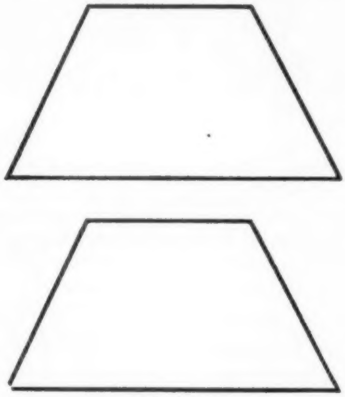

FIG. 27.

shows very well when the figures are cut out and moved about to assume various positions. The upper figure seems larger, because its long side is brought into contrast with the shorter side of the other figure. Similarly, a square resting on a corner seems larger than one resting on a side, because we then contrast the diagonal with the side. Fig. 28 on the following page presents another illustration of the same principles; the lower figure seems to be distinctly the larger, and the contrast is emphasized because it is thrown entirely to one side of the figure. In judging areas, we cannot avoid taking into account the lengths of the lines by which the areas are limited, and a contrast in the lengths of these is carried over to the comparision of the areas. We judge relatively even when we most desire to judge absolutely. Relative distinctions and

the perception of relation seem to be more natural and significant than absolute ones. We cannot view the part as unrelated to the whole. This is a widely applicable princi-

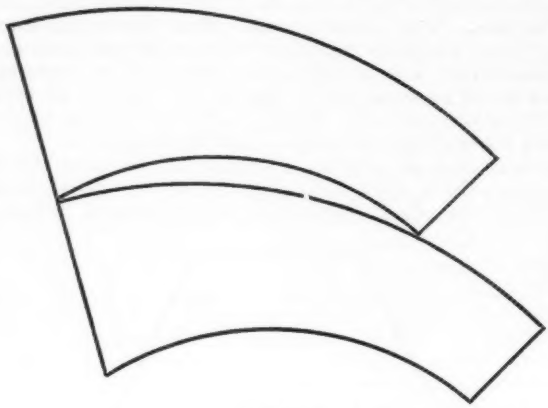

FIG. 28.

ple and is suggested as a convenient guiding principle by which the study of such illusions of sense may be profitably directed.

A STUDY OF INVOLUNTARY MOVEMENTS.

(With the assistance of HELEN WEST.)

The dictum that thought is repressed action most readily finds illustration in conditions of the nervous system varying somewhat from the normal. It is easier to detect the action of not definitely recognized laws in extreme forms than in average ones. The modern view of morbid action, however, emphasizes the close relation of the abnormal to the normal; there exists in the latter in germ and to a limited extent, what is full grown and characteristic in the former. If, under great excitement and extreme fascination of the attention and in favorably constituted individuals, the involuntary movements are pronounced, the rudiments of these movements should be demonstrable in the average individual under normal conditions. For this purpose delicate apparatus may be requisite, and a variable amount of success is to be expected. The question of apparatus is of importance, and our present study aims to do little more than describe the apparatus and illustrate what results may be obtained therewith.

The apparatus is so simple that a brief description will doubtless be sufficient to convey a clear idea of its mode of action. There is first a piece of plate glass (see Fig. 29)

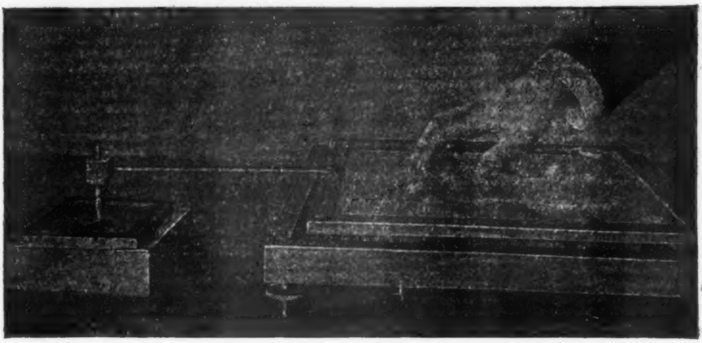

FIG. 29.

fiifteen inches square, resting in a stout wooden frame; this frame is mounted on three adjustable brass legs, raising it an inch or so from the table. By means of the screw-adjustments of the legs, the plate glass is brought into exact level. Three brass balls, which must be very perfectly turned and polished spheres, about three-fourths of an inch in diameter, are placed in the form of a triangle upon the plate; upon these balls rests a very light crystal-plate glass, fourteen inches square, mounted in a light wooden frame. On the upper surface of this plate is placed a piece of paper to hide the balls, and on the paper we lightly rest the finger-tips of our hand. It is almost impossible to keep the plate from all motion for more than a few seconds; the slightest movement of the hand slides the upper plate upon the balls. To maintain the apparatus in working order it is necessary to keep the glass and balls well polished by rubbing with a cloth and a little oil.

The recording of the movement is equally simple. To the light frame is attached a slender rod about ten inches long, bearing at its end a cork; piercing this cork is a small glass tube and in the tube there is a glass rod snugly fitting the tube and drawn to a fine point. The point of the rod traces the movement of the hand with great accuracy, and, not being rigidly fixed, can accommodate itself to all irregularities of movement or of the writing surface. A piece of smoked paper stretched over a glass plate, upon which a record is

made, and a large screen to prevent the subject from seeing the record, complete the apparatus. This apparatus enables us to record all movement in the horizontal plane, and, inasmuch as its chief purpose is to write slight involuntary movements, we have given it the name of the *automatograph* and may speak of such a record as an *automatogram*.

The type of an experiment is the following. The subject places the finger-tips of his extended right hand upon the glass; he is told to hold the arm still and pay no attention to it. He is asked to read some lines or colors, or to count the beats of a metronome; this naturally engages his attention. When all is in readiness the operator drops the glass rod into the tube, and the record begins. When the subject has been occupied in this way for a minute or so, we have, as a rule, a very clear record of the direction of his attention in the automatogram.

In order to have a test by which to compare the relative sensibility of different persons for movements of this kind, we arranged to have a number of persons go through a series of tests, a typical result of each of which will be figured. Each experiment occupied from about three-fourths to two minutes, and when possible we noted the progress of the record for each 15 or 30 seconds.

A series of patches of color 5x20 mm. were placed in horizontal rows on a vertical wall about ten feet distant. The subject was required to read aloud the names of the colors. The general tendency is for the hand involuntarily to move toward the colors with a variable degree of constancy, rapidity and directness. An average result is shown in Fig. 30. We have another record, lasting but 45 seconds, but covering 6½ inches, which in extent and directness is the most remarkable of our records. The appearance of the line is similar to that of Fig. 30, but with several points at which the line is almost directly toward the colors.

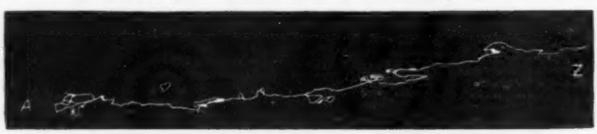

FIG. 30. →

Reading colors. Time, 95 seconds. A indicates the beginning, and Z the end, of each record. The arrow everywhere indicates the direction in which the object attended to was situated. When the numbers 1, 2, 3, 4, occur, they indicate the point of the record at 15, 30, 45 and 60 seconds after the beginning of the record.

On two occasions the subject who gave us this striking record evidenced the action of the attention in another, equally striking way. There were three rows of colors which

were read; the first one from left to right, the second from right to left, and the third from left to right; the involuntary movements correspond to the movement of the attention, as is vividly shown in Fig. 31.

FIG. 31. ⟶

Reading three rows of colors; the movements closely following the attention. Time, 90 seconds.

These are certainly striking proofs of the ease with which, in sensitive subjects, the hand involuntarily follows the movements of the eye.

A second test consists in substituting the reading of a

printed page for the colors the results are quite similar. Fig. 32 represents a typical result.

FIG. 32. ⟶
Reading from a printed page. Time, 45 seconds.

We next pass on to cases in which the attention is directed to sounds. We set going a metronome, and ensured the subject's attention to it by having him count the beats. The usual rate was 140 strokes per minute. Here, again, we find two types of involuntary movement: the one a moving toward the sound, represented in Fig. 33; the other a keeping time with the beats, not accurately at all, but in a general way, as is shown in Fig. 34. When we consider

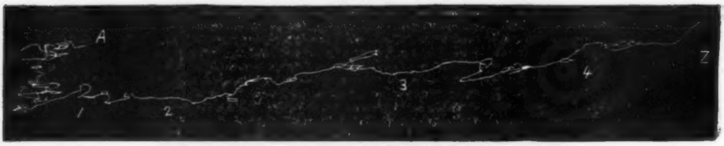

FIG. 33. ⟶
Counting strokes of metronome at 140 per minute. Time, 70 seconds. Also illustrates slight hesitation before movement towards metronome begins.

FIG. 34. ⟵
Counting strokes of metronome. Shows movements to and fro with the strokes.

how strong is the tendency to keep time to enlivening music, it will not surprise us that we are able to record these slighter and more unconscious movements to simple time beats. We frequently performed this experiment by placing

the metronome first in front of, and then behind the subject, and the contrast between the direction of the lines is, as a rule, quite striking.

We recorded a similar experiment for sight, by substituting for the metronome a silently swinging pendulum, the oscillations of which were to be counted. Again we observe the two kinds of records, the second, as before, being considerably less frequent than the first. These are given in Fig. 35 and Fig. 36. A pair of records derived from this form of ex-

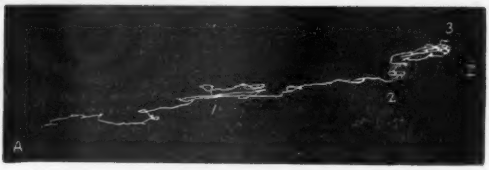

FIG. 35. ⟶
Counting oscillations of a pendulum. Time, 45 seconds.

FIG. 36. ⟶
Counting pendulum oscillations; shows movements to and fro with the oscillations. Time, 80 seconds.

periment well illustrates the extremes of rate of movement: one subject moved 11 inches in two minutes, another $1\frac{3}{8}$ inch in the same time, though in both cases the motion was regularly toward the point of attention, the swinging pendulum.

Our next experiment approximates closely that of the muscle reader. We directed the subject to hide a knife at some part of the room not near the center, and immediately thereupon took a record upon the automatograph, the subject

thinking of the knife. In some cases this experiment was unusually successful, in others fairly so; the direction of movement usually closely approximated the direction in which the knife lay. Fig. 37 represents a fair result. A

FIG. 37. →
Thinking of hidden object. Time, 30 seconds.

quite similar experiment consists in directing the attention to some prominent building or locality in the neighborhood, not by actually looking toward the place, but by voluntarily thinking of it. We have many very excellent examples of such records. Fig. 38 will serve as a type of the more successful ones.

FIG. 38. ←
Thinking of a building in the direction of A to Z. Time, 120 seconds.

This does not exhaust the methods of attracting the attention but it illustrates our chief modes. Reading to a person from different parts of the room is often successful. Quite an interesting form consists in having the subject's attention change in the course of an experiment to different localities, as by having him read from a book carried about by an assistant. Such a result is shown in Fig. 39, in which we have an irregular figure closing in upon itself and clearly indicating the circular movement of the book.

We often succeeded in distracting a subject's attention by a noise in another portion of the room, the hand moving toward the source of the noise. We also recorded the involuntary start that occurred when a ball was suddently dropped upon the floor.

The figures given will sufficiently illustrate the nature of

the results obtainable with the aid of our automatograph and it remains only to notice a few general points regarding them.

Fig. 39. Reading from a page moved about in a circle.

It would be interesing to determine by this method the relative degrees of muscular accompaniment for these different kinds of attention; but our methods are not as yet sufficiently refined to solve this problem; the result seems to vary with individuals and with the sense organ engaged. As a preliminary result it may be worth recording that a number of measurements yielded an average rate of movement of about two inches to the minute toward the object thought of.

Of great importance is the nature of the individual differences in these experiments. Our normal experience would naturally anticipate a difference about as characteristic at least as that of hand-writing. Any minute discussion of the point would be obviously premature, but in general it seems possible to arrange these differences in types. We should distinguish between those who move rapidly and directly, and those who move slowly and circuitously; between those in whom the movement quite exactly follows the line of attention, and those in whom it does so only approximately or irregularly. Instances of such distinctions have already been indicated.

We add Fig. 40, which may be contrasted, in regard to the

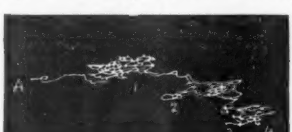

Fig. 40.

Counting oscillations of a pendulum. 1, 2, 3, 4 indicate the points 30, 60, 90 and 120 seconds after the start. Time, 120 seconds. Illustrates small and indirect type of movement.

character of the curve with Fig. 31; the latter shows directness of movement and great extent, the tracing rarely becoming confused by the hesitancy of the subject, while, in Fig. 40, the movement is slow and the record involved by continual retracings of the path of movement. Another important distinction relates to the time at which the most significant movements occur, and mainly whether the first impulse is toward the object of attention followed by much hesitation of movement; or whether at first there is little movement followed, after fatigue, by the movement determined by the attention. We have many more of the former type than of the latter; one of the former is presented in Fig. 41. Figs. 33

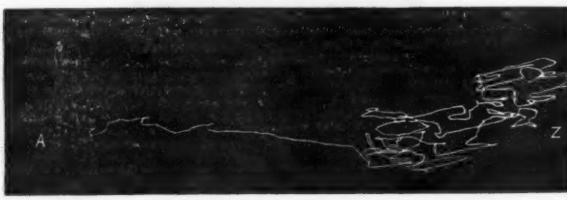

FIG. 41. ⟶

Counting beats of metronome. Illustrates first impulse toward object of attention followed by hesitation. Time, 90 seconds.

and 38 partly illustrate the reverse tendency. We might further distinguish between those subjects who show the direction at each portion of the tracing and those who show it only here and there.

These types may possibly suggest what kinds of involuntary movements best subserve the purposes of the muscle reader; all alike illustrate the general line of tendencies which he utilizes.

A very natural query relates to the possible influence of the position of the arm and body, and also of such other factors as the pulse and respiration upon the character of the tracing. The main distinction in regard to the position is whether the arm is (1) held straight out from the shoulder in a line with the trunk, or (2) at 90° from this position, or (3) in an intermediate position. In the first of these, movements toward the front are obviously easier to make than movements toward the rear, and in both (1) and (2) movements toward the body are easier than those away from the body. By changing the position of the object to which the attention was given, we could thus favor or interfere with the tendency to involuntary movement in certain directions. In our experiments, we allowed the subject to assume a natural and comfortable position, which was usually an intermediate one,

with the arm not fully extended. This position allows movement in all directions, though it is still true that movements toward the front and toward the body are favored above movements toward the rear and away from the body. The direction of the attention is thus sometimes partially obliterated in some subjects, but in most of them it appears in spite of this tendency. This factor deserves a more special investigation. While respiration may have some effect, we are inclined to regard it as very small, and the pulse as not entering into consideration at all; for, in order to get the tracing of pulse and respiration (by other apparatus) with equal distinctness, we had to magnify them very considerably.

The question of the precise significance of these movements is largely dependent in the testimony of the subjects. While there are individual differences in this as in all other respects, the consensus of the verdicts might be thus expressed: at times we become aware that our hand has moved, but rarely of the direction of its movement; the movements are sometimes unconscious but always involuntary, there is often great surprise at the result. The one objective test we could apply was to intentionally simulate these movements and the result was measurably different from the genuine involuntary record.

It is hardly necessary to enlarge upon the bearings of these experiments upon the processes of muscle reading and kindred phenomena. They indicate the close connection of mind and muscle, and in demonstrating the extent of recordable automatic movements, suggest the many other and subtler means by which we may give to others some notion of what is going on in our own minds.

Observations on the Absence of the Sense of Smell.

(With the assistance of Theodore Kronshage.)

The subject of our observations is a Mr. E., aged 21 years, a student of this University, who is deprived of all the sense of smell. The defect is probably congenital and of nervous origin. As Mr. E's. knowledge of his defect was denied from such occasions as would occur in every-day life, our first step was to test the degree and extent of the anosmia. We approached various substances to his nose asking him to inhale them and report the result; we tried in this way strong liquid solutions of wintergreen, bitter almonds, ether, alcohol, ammonia, cinnamon, camphor, etc. Camphor produced very slight if any sensation and the same is true of wintergreen and cinnamon. Bitter almonds, ether, and most markedly ammonia, produced a sharp, more or less stinging sensation

in the nose. Alcohol was described as sweetish like perfumes. We next tried several substances in pairs to determine how far, when first told which was which (not by name but by calling one *A* and the other *B*), he could distinguish between them, and, as a check against unconscious bias, experiments in which one of the pair was distilled water were introduced. This precaution was quite necessary, for it happened that when bitter almonds and water were compared in this way he mistook them three times in five trials, though professing te get some sensation from the bitter almonds when presented alone. The water, however, was frequently recognized by its entire absence of any sensation of smell. Such distinctions therefore as are perceived by him are by no means altogether clear. With the pair, ether and water, eleven trials resulted in eleven correct answers, the point of distinction being that "ether opens up the throat like peppermint." With wintergreen and bitter almond, the latter yielding the distinctive effect, there were only 3 errors in 18 trials. With ether and ammonia both giving decided sensations but of somewhat different nature there were 2 errors in 8 cases; this may however have been due to over stimulation, as the substances used were so strong that neither of us could take them to the nose with comfort. Ammonia was described as immediately affecting the nose, ether as going back to the throat and affecting it. With wintergreen and cinnamon, neither yielding any definite sensation the result proved to be mere guesswork, and the same is true of cinnamon and camphor.

Inasmuch as the sensation arising from the inhalation of alcohol was described as similar to that of perfumes (alcohol being an ingredient of these) we ascertained how far the presence or absence of alcohol could serve as a means of confusion or indentification of substances. We made a strong solution of wintergreen and of cinnamon in alcohol, and from each of these Mr. E. obtained a similar but pronounced effect. The attempt to distinguish between the two, however, resulted in as many failures as successes. We next compared pure alcohol with wintergreen dissolved in alcohol, and no difference except of intensity was observed. To complete the proof we made a solution of wintergreen in water and in alcohol. The latter gave a distinct sensation, the former almost nothing. In all the eight trials the two were correctly distinguished.

The next point tested was whether distinctions of intensity within a perceived range of sensations were obtained. We tried strong and weak alcohol with the result that in all cases (eight) they were correctly distinguished from one

another; the sensation was described as a "sweetish taste in the mouth."

In the above results indications were given that Mr. E. was less than normally sensitive to irritants. To measure this difference we determined how many drops of very strong ammonia must be added to 100 cc. of water (1) to produce a sensation, (2) to make it objectionably strong. We obtained the characteristic effect with but one drop in 200 cc. and even one in 300 cc.; while Mr. E. needed 2 drops in 100 cc. Eight drops in 100 cc. made it very objectionable to us, but he said it was like some perfumes, and it took 23 to 25 drops to produce an objectionably strong sensation.

We next tested the sense of taste. A preliminary survey served to show that the sense was present and presumably in a normal degree. To complete the test we compared his taste with ours for sugar, acetic acid and quinine. We found about the same measure of sensitiveness for Mr. E. and for us; and found nothing differing from the normal in any respect.

We proceeded to investigate those mixtures of smells and tastes, which make up most of the sensations obtained during eating. We took the ordinary flavoring syrups of commerce, lemon, vanilla, currant, orange, strawberry and raspberry. From all these Mr. E. obtained only a general sweetish sensation with no distinction between them except from the lemon which was in the main distinguished by the mixture of sweet and sour. He could in part tell them by their different degrees of sweetness, but when presented in proportions in which they seemed to us equally sweet all distinctions were impossible to him. The tests showed as many wrong as right answers. It so happened that some of these substances fermented and these he could at once detect as different from the others and also the more fermented ones from the less so. A series of candies with some of the above flavors yielded corroborating results. It should be understood that all these substances were tasted.

Next, a series of spices was tried with the following results:

Mustard; a sharp sensation on the end of the tongue; not recognized.

Pepper; same effect but stronger.

Coffee; not recognized, a slight taste.

Cinnamon; recognized, sweet and sharp.

Broma; sweet.

Cloves; recognized, taste distinct but not describable.

Thyme; sharp, bitter, something like cloves.

Tea; no effect at all.

Anise; sharp, bitter, unpleasant.

Caraway Seed: mild, sweetish, and salt.

Ginger; not recognized, burns.

Mustard Seed; burns decidedly.

Citron; recognized by its feeling on the tongue, sweet.

In brief some were recognized by secondary qualities, but those that we recognize by flavor were not differentiated. A separate series was tried with tea and coffee, and one with ginger and cloves. Neither of either pair was distinguished from the other; the latter were both called sharp but with no distinction between them.

We also tested Mr. E.'s temperature sense, at about 15°, 30° and 60° R. At all these points his sensibility was as good as ours, differences of 1° being everywhere recognized. The test was made by taking a mouthful of water heated to the required temperature and then throwing it out.

The great importance of these observations lies in the analysis they enable us to make of the complex of sensations obtained in the mouth and nose. In Mr. E.'s case taste is normal, the temperature sense is normal, the tactile sensibility is present (though as far as irritants are concerned, to a diminished extent,) while smell alone is absent. Accordingly we may conclude that such distinctions as Mr. E. fails to make are in us due to the sense of smell, and such as he makes are due to other senses. *The results conclusively show that a great many of the mouth-sensations, which we ordinarily speak of as tastes, are really due to smell.* The distinctions between tea and coffee, between all the various flavors that make the difference between candies and sugar, between various syrups and so on—all these are lost. That the absence of marked sensations during eating should lead to a relative neglect of such sensations is natural; Mr. E. is perhaps on this account less sensitive to other mouth sensations.

Mr. E.'s defect was observed by members of his family as soon as the sense of smell could be tested. He has no recollection of ever having smelled and his family agree that he never gave evidence of such sensation. It is certain that he could not smell when a very small boy. He gets no sensations from flowers, perceives no difference in the taste of his food when afflicted with a cold, and observes no distinctions in the various kinds of sweet things of which he is fond. He perceives no distinction between tea, coffee, and hot water flavored with milk and sugar and has come to take the latter as his every-day drink.

Mr. E.'s case is especially interesting because his mother has a similar defect. Mrs. E. however at one time possessed the sense of smell and distinctly remembers the sensations

derived from odors and her use of odorous substances. She seems to have lost the sense when about 13 or 14 years of age. It is definitely established that she is the first and only one of her family or her husband's family to show this defect. She has two sons and two daughters besides Mr. E., all of whom are normal as regards the sense of smell. Some of the more typical experiments performed upon Mr. E. were also tried by Mrs. E. with strongly corroborative results. Ammonia alone had a marked effect; the same confusions were made by Mrs. E. as by her son. She is likewise deficient in distinguishing "by taste" between flavoring extracts and similar substances.

Classification Time.

(With the assistance of George W. Moorehouse, Fellow in Psychology, and Mildred Harper.)

An extremely frequent and important mental process consists in the reference of some item of knowledge to some familiar class. The assimilation of knowledge involves the appreciation of the relation of the new fact to the general body of acquired knowledge. In order to maintain in an orderly and accessible form our mental acquisitions it is necessary to view each item of information as belonging to such and such classes. Psychologists have variously analyzed this process; some express their views by picturing the mind as possessing a number of apperceptive instruments and using now one and now another of these according to the nature of the object to be assimilated, or, again, as a series of lanterns each of which has its own focus and field of illumination. However we may view the process it is clearly essential to the acquisition of knowledge, and it is strange that the study of the time-relations of this process has been hitherto so largely overlooked. The present contribution considers the time of a special form of such classification.

As a distinctive and readily studied form of such reaction we selected the reference of a common word to its grammatical class. We further limited the problem by selecting the following ten nouns, ten verbs and ten adjectives and confining our reactions to calling the proper part of speech to which one of these words belonged: house, cat, book, ship, ant, sun, lake, doll, man, girl; push, have, cut, mix, go, die, look, sit, jump, touch; tough, wet, good, blue, low, bad, high, thin, hot, black.

These words were chosen as familiar, distinctive and monosylabic representatives of their classes. The full list of words which he might be called upon to classify was always read to the subject before each kind of experiment. We reacted to a

spoken word by a spoken word. The apparatus by which this was accomplished consisted of a bit of wood held between the teeth connected with one arm of a lever the other arm of which bore a metallic point for electrical contact. A spring connected with the lever tended to pull the bit from between the teeth, and according to the adjustment to make or break an electric circuit. Both subject and observer used an instument of this kind, the instruments being so connected with the chronoscdpe that the release of the bit from the mouth of the observer started, and a similar action from the subject stopped it. The necessary act of separating the teeth that accompanies articulation is here taken as the point of measurement. The apparatus is fairly satisfactry, and so long as the results are used mainly for relative purposes the error involved in its use may be neglected. A perfect apparatus whereby the utterance of a word will start or stop a chronoscope is still a desideratum.

The entire process may be viewed as consisting of the following steps: (1) the hearing of the sound uttered, (2) the recognition of the word, (3) the reference of the word to its class, (4) the summoning of the term describing that class, (5) the muscular innervation accompanying the utterance of the term. In order to determine the time of the purely mental process involved in expressing the fact that a certain one of ten words is a noun, or verb, or adjective, it was necessary to measure separately the time of the mechanical steps. The simple reaction involved evidently consists of steps (1) and (5). This time for each of the three subjects we found to be 190^σ, 195^σ and 199^σ respectively. It is naturally somewhat long for a simple reaction because the muscular contractions by which it is signalled that the impression has been received are complicated, and because the moment at which the chronoscope starts may slightly precede that at which the soundwave reaches the subject's tympanum. In all these simple reactions both observer and subject used what seemed to be the easiest vocal utterance; it consisted of a violent expiration, the result resembling the sound *eh*.

We further need in order to measure step (3) in which we are particularly interested a process involving steps (2) and (4) as well as the simple reaction. It seemed impossible to devise any simple process of the kind, but the process of repeating a word sufficiently approximates it for our present purpose. This process clearly involves in addition to the simple reaction, the recognition of a word and the summoning and utterance of a word. The only question would be whether the summoning of a term denoting a grammatical class is of equal difficulty with the repetition of a recognized word, but

as both are very familiar and somewhat mechanical processes, their time relations can hardly be very different. The repetition time for the three subjects was as follows: 367^{σ}, 280^{σ}, 333^{σ}.

The experiments in which words were referred to grammatical classes were of the following kinds: (1) the subject was to tell whether the word was a noun or verb; (2) the same distinction regarding nouns and adjectives; (3) the same distinction regarding verbs and adjectives; (4) the same distinction regarding nouns, verbs and adjectives. Experiments are grouped in sets of twenty each. In fact from 22 to 25 observations were taken and those most divergent from the average of all were discarded until 20 were left. A new average of these 20 was entered. The following table gives for each of the three subjects the average time of the several reactions together with the number of sets of which it is the average.

Subject.	Simple.	Repeat.	'Noun-Verb.'	'Noun-Adj.'	'Verb-Adj.'	Average of 'N-V,' 'N-A,' and 'V-A.'	'Noun-Verb-Adj.'	Mental Time.
J. J.	190 (18)	367 (10)	599 (16)	595 (10)	593 (10)	596	667 (11)	71
G. W. M.	195 (17)	280 (17)	628 (13)	597 (10)	679 (10)	635	678 (9)	43
M. L. H.	199 (10)	333 (11)	612 (15)	550 (10)	568 (10)	577	589 (10)	12
Average.	195	327	613	581	613	602	645	42

Combining the results of the three observers we obtain as the result the fact that with a reaction time of 195^{σ}, and a repetition time of 327^{σ}, it takes 603^{σ} to determine whether one of a set of words belongs to one or the other of two grammatical classes (the mental portion of this process consuming 276^{σ}), and that it takes 645^{σ} to refer a word to one of three grammatical classes.

It hardly seemed worth while to calculate the mean variation of these observations, but to satisfy ourselves regarding the regularity of the results we calculated it for the three most typical sets under each kind of reaction. Expressing

the mean variation for these three sets as a percentage of the general average time for the kind of reaction, we obtain the following table.

Subject.	Simple.	Repeat.	'Noun-Verb.'	'Noun-Adj.'	'Verb-Adj.'	Average of 'N-V,' 'N-A' and 'V-A.'	'Noun-Verb-Adj.'
J. J.	8.0	8.4	11.0	10.3	9.9	10.5	9.5
G. W. M.	16.5	13.2	11.8	13.4	10.9	12.0	13.4
M. L. H.	20.9	9.7	8.2	5.6	7.6	7.0	8.3
Aver.	15.0	10.3	10.3	9.9	9.6	10.0	10.4

This table indicates a very fair degree of regularity with the exception that, markedly in the case of M. L. H., and to some extent in the case of G. W. M., the variation for the simple reaction is large. This is clearly due to the fact that experimentation began with the simple reaction alone so that this variation indicates absence of practice in reaction work.

The results are, however, effected by inequalities of practice. This is particularly true of the time for 'Noun-Verb-Adjective' distinctions which observations were made last and were therefore most benefitted by the practice gained in the former distinctions. It is probable, therefore, that the difference in time, 42^{σ} between the two processes is too small. This appears more clearly in considering the results for each subject. For J. J., who began with most practice in this kind of observation and whose time for the three classes of distinction, 'Noun-Verb,' 'Noun-Adjective' and 'Verb-Adjective,' show greatest constancy, the difference in question is largest, 71^{σ}. For G. W. M., who began with some practice in reaction experiments the difference is intermediate, 43^{σ}, and would be greater were it not for the special and temporary difficulty he encountered in distinguishing verbs from adjectives, the difference between the average of the 'Noun-Verb' and 'Noun-Adjective' and the 'Noun-Verb-Adjective' being likewise 71^{σ}. While for M. L. H., who began with no practice and showed steadily decreasing time for each successive kind of reaction attempted, the difference in question is but 12^{σ}. It is probable then that the time

for J. J., 71σ, is a more typical result than the general average, 42σ.

The relative difficulty of the three pairs of distinctions, 'Noun-Verb,' 'Noun-Adjective' and 'Verb-Adjective' probably varies with different individuals; in the present study it is also affected by differences of practice; on the whole, however, our results favor the view that the three are of practically equal difficulty.

The increase in time in passing from two distinctions to three is an interesting illustration of the effect of the mental attitude on reaction times. The process involved is the same in both cases, to decide, for example, that *man* is a *noun*, but this decision requires more time when the word in question may belong to one of three grammatical classes than when it may belong to one of only two. Our results indicate that all of these processes are quite complicated and that their time-relations depend upon the accessibility of very familiar items of knowledge.

Regarding possible differences between the several words, they may vary with individuals; extended results would be needed to clearly show their existence. It is interesting, however, to observe that taking the average reaction of each word in all the three kinds in which it occurs we find among nouns "ship" was most quickly classified by all three subjects; another easy word was "man"; especially difficult nouns that were "doll,' "ant" and "cat"; among verbs "sit," "jump" and "go" were relatively easy, "have" and "cut" relatively difficult; among the adjectives "good" was particularly easy, "wet" and "blue" fairly so, "high" particularly difficult, "bad" and "hot" fairly so. It should be noted, too, that this difficulty may in part be due to difficulties of recognition and pronunciation.

FINDING-TIME.

(With the assistance of WINIFRED SERCOMBE and LUCY M. CHURCHILL, [MRS. FRANK T. BALDWIN].)

We have employed the term "finding-time" to denote the time occupied in the process of finding a given object within a given field; we recognize with what different facility and rapidity different persons perform such tasks, and even in the same individual the time is subject to variation. We have all experienced the difficulty of finding an object even when it is plainly in sight, and have wondered at the long time necessary to find a quotation in a volume and the like. In this process we carry with us a mental picture of the object sought and we react when the subjective corresponds to the objective picture. The ability to recognize one of a number of objects as the one desired is certainly a useful trait and

may perhaps be a convenient test of mental alertness. It is this process that we desired to study and to measure. The difficulty of finding an object varies with several factors, the most important of which may be thus summarized: (1) the number of objects amongst which one is sought; (2) the nature of the object; (3) the minuteness or complexity of the differences by which the one object is distinguished from the others; (4) the degree of probability (which may amount to certainty) that the object sought is within the given area.

In our study the objects sought were the letters of the alphabet; the method of finding them was as follows: The letters (plain capitals about 4 dioptrics or in the average 6.5 mm. square and very closely conforming to the Snellen types) were gummed on a card which was in turn fastened on a block, and were seen through square openings in a black screen. These openings, 25 in number, were 11 mm. square and were each separated by 19.5 mm. above and below and to each side from the neighboring opening; this screen was laid on a glass plate mounted in a square frame that slipped over the block and (inside) was about 15 mm. larger each way than the block. The block contained four alphabets distributed by a chance arrangement, and according as the frame was moved to the upper left hand, the upper right hand, the lower right hand, or the lower left hand corner, one or another of these alphabets was seen through the openings in the screen. The arrangement may be made clear by reference to the letters below. Here each different kind of type represents an alphabet and it will be clear from this how a simple movement was sufficient to bring to view through the openings in the screen another alphabet. In the original all the letters are of course alike, and distributed by a chance arrangement. Connected with the frame by means of two iron uprights was

C	S	X	O	R	L	T	M	O	T
F	I	R	C	O	G	A	R	C	H
I	J	P	A	H	B	B	C	F	K
G	S	U	N	S	L	I	A	E	J
Z	G	N	V	K	W	S	E	W	Z
M	F	Z	B	W	P	B	D	F	K
J	R	E	D	V	F	D	H	Y	N
D	Z	Y	U	J	E	V	X	H	V
G	Y	L	X	U	I	M	F	A	U
L	Y	X	M	T	W	K	T	N	O

a head piece similar to that of a stereoscope against which the subject rested his head and through two openings in which he viewed the letters. Across these openings is a hard-rubber flap which may be quickly withdrawn by bringing into action a strong spring. As this flap opens it closes an electric circuit and thus starts the chronoscope.[1]

An observation was conducted as follows: the frame is set for a certain alphabet; the operator announces the letter to be found (this also serves as a signal) and shortly thereafter he pulls a cord releasing the spring and allowing the subject a view of the letters. As soon as the desired letter is seen the subject presses a key and stops the chronoscope. To test whether the subject knows where the letter is situated he keeps a record of each answer. The positions were indicated by lettering the double rows A, B, C, D, E, and the columns 1, 2, 3, 4, 5, so that A1 would indicate the upper left-hand corner, D5 the lower right hand corner and C3 the centre letter. In the first experiments 25 letters were thus shown (Q was omitted), but this could be reduced to a four-square (16 letters) by covering over either the row A or E together with either column 1 or 5. Throughout the experiments except when distinctly stated otherwise, the subject was assured that the letter sought was present.

The following table represents our average results for the three observers separately and together.

	Finding one of 25 letters.	Placing one of 25 letters.	Finding one of 16 letters.	Placing one of 16 letters.	Finding one of 25 letters, with 9 letters absent.			Finding two of 25 letters.
					Average.	Absent.	Present.	
J. J.	(20)582	(13)309	(10)413	(7)316	(10)915	1085	817	(7)1445
W. S.	(16)485	(11)210	(10)302	(10)175	(10)649	722	610	——
L. C.	(13)640	(10)355	(10)428	(10)288	(10)761	1048	651	(18)1836
Aver.	569	291	381	260	775	952	693	1640

The numbers in parentheses indicate the number of sets of 20 observations from which each average was derived; the

[1] The essential features of this apparatus as well as of the problem investigated were suggested by Prof. G. Stanley Hall and were elaborated in conjunction with him at Johns Hopkins University some years ago.

other numbers represent the average times in σ=.001 second. We see that it took on the average 569σ to find one of 25 letters and 381σ to find one of 16 letters. The process is thus quite complicated and is very difficult at first, the stage of initial practice being quite marked and the first few sets yielding very long times. Considerable of this time is consumed in the process of accommodating the eyes to the plane of the letters and bringing them clearly into view. We considered that this time would be measured by measuring the time needed to see what letter occupied a certain position amongst the twenty-five. Instead of calling a letter and reacting when its position was seen, a position was called, for example A1, C3, etc., and the subject reacted when the letter occupying that position was recognized. The subject here knows just where to look and, although this time includes the recognition of the letter as well, we should remember that it is probably fair to exclude this element from the time of finding letters, the finding time strictly applying only to the process of search. While therefore only an approximate elimination of the mechanical process is obtained by subtracting the "placing time" (as we shall call this latter step) from the "finding-time," yet this difference very fairly represents the distinctive part of the finding process and is remarkably alike in the three subjects, 273σ, 275σ and 285σ.

The effect of the number of objects amongst which one is to be sought, and of the larger field is illustrated in the difference of time between finding one out of 16 and one out of 25 letters; this is on the average 188σ and in the individuals 169σ, 183σ and 212σ. The ratio of the times to find one out of 25 and one out of 16 letters thus increases in the proportion of 1.55 to 1 which is just the ratio of 25 to 16.

In "placing" a letter, that is, in recognizing what letter occupies a certain position, it is obvious that the time should be little, if at all, affected by the number of places, and the slight difference between the values found for placing one of 16 or one of 25 letters, 260σ, and 291σ is probably due to the fact that the former sets represent a more advanced stage of practice than the latter.

The next variation presents an interesting difference; 16 letters are present and, as before, these change with every observation, but instead of calling only for those letters that are present, any one of the 25 letters may be called for, and if not present the subject reacts as soon as he is convinced of its absence. The average result of all the experiments performed in this manner is 775σ; this, however, is not as significant as the result we obtain by considering separately those cases in which the letter to be found was present and those

cases in which it was absent. That it should take longer to go through a series of 16 letters and determine that a certain one is absent, than to determine its presence, is to be expected; the difference is certainly great whether we compare it with the finding time of one of 25 letters or more properly with the finding time of one of 16 letters. It takes 571σ longer, or 2½ times as long, to determine that a given letter is not among a group of 16 than to find it if it is present. But while it takes 381σ to find one of 16 letters when the subject knows it is there, it takes 693σ to perform precisely the same process when there is a chance (strictly when there are 36 chances in a 100) that the letter he is seeking may be absent. This result most strikingly illustrates the effect of the fore-knowledge of the subject upon the time of mental processes; the apparently simple process of comparing an objective with a subjective image varies its character according to the underlying connection by which the process is accompanied. This result, too, appears in the fact that, while in finding letters all of which are known to be present, an error is exceedingly rare when the letters may be absent. Errors are quite numerous and consist in declaring a letter that is present to be absent.

In certain processes it is relatively easier and quicker to do two things together than to perform them separately; this being due to an overlapping of the mental processes. There is a division of the attention among the several mental tasks so that the time needed for the whole is considerably less than the sum of the times needed to do each separately. In other cases the attempt to perform processes together seems to result in a mutual inhibition or confusion and a loss of time and energy. As a small contribution to an investigation of this problem we determined in two subjects how long it takes to find two letters among 25 and to note their positions. The two letters were announced beforehand and as soon as both were found the subject reacted. This proved to be a very difficult and often confusing process; it took on the average 1640σ which (for the two persons under consideration) is 418σ longer than twice the time needed to find one letter. This may serve as an index of the loss of energy in attempting to have two processes before the mind simultaneously.

While our results are not sufficiently numerous or free from great variation to warrant detailed inferences, yet there are two such questions of detail the importance of which justifies even the mention of the imperfect information we are able to give. The first relates to the difference in ease in recognizing the various letters. That such differences occur has been shown by more suitable methods. Our results show considerable variation; for one subject the range is from

393^σ for W to 557^σ for T; for another from 487^σ for S to 719^σ for L. On the whole the three letters most quickly found were S, O, and W; and the four least quickly found L, J, H and T. If we ventured to divide the alphabet into three groups of easy, medium and difficult letters, our lists would read: 1. S, O, W, N, D, C, E, I; 2. X, B, Z, G, M, Y, A, R, B; 3. K, U, F, V, T, H, J, L. It must be remembered however that no great weight is to be placed in this detailed result. The second question involves the query whether the letters nearer the centre of the block are more readily found than those away from the centre. Our results are unfortunately not recorded in such form as to readily allow of the determination of this point; but we compared the times for all the letters found in positions B3, D3, C2 and C4, that is, in a diamond about the central letter C3, with those for finding the four positions furthest removed from the centre, A1, A5, E1, E5. Our result showed a slight excess of time for finding the peripheral letters, an excess too slight perhaps to be recorded were it not for its constancy in all three individuals.

This first attempt to gain a deeper insight into the mental process of finding certainly leaves untouched the larger number of important and suggestive queries attached to it, and yet the results obtained are sufficiently clear and consistent to justify the promise of future investigation.

Some Anthropometric and Psychologic Tests on College Students.—A Preliminary Survey.

(With the assistance of George W. Morehouse, Fellow in Psychology.)

During the fall of 1890 it was decided to ask the students in the general class in Psychology to lend themselves to series of physical and psychological tests with a view of interesting the students in such tests as well as acquiring a body of statistical material which when sufficiently extended and properly compared with other statistics might prove of considerable value.

The experiments were not extensive in character but they served to bring out the difficulties in this line of work, and the publication of the present fragmentary results[1] is ventured in the hopes of furthering similar observations elsewhere.

The tables given below require more or less explanation and comment. The physical measurements of the men are in the

[1] Simple and few as the tests were they required about 50 minutes for each student. If the tests could be arranged so that several persons might be tested together without interference a great saving of time would result.

main those regarded as most important by Mr. Galton, and were made with the intention of correlating mental with physical characteristics. The apparatus employed was very simple and hardly needs description. The dynamometer is of the Feré pattern, made by Cullin, Paris. Similar measurements for the women were obtained through the courtesy of Miss Ballard, in charge of the Ladies' Gymnasium, but were too few in number to warrant tabulation.

In four cases the measurements made by Mr. Galton upon miscellaneous Englishmen are exactly repeated upon these college students, and the results indicate in so far as such few results can indicate, a superiority in favor of the college students.

The sensibility tests were selected to quickly yield a few typical results. Like all such observations the chief difficulty lies in the fact that the subjects are not used to accurately observing their sensations, so that a relatively brief practice would in many cases alter the result. The æsthesiometer employed was that described in this JOURNAL (Vol. I, p. 552). It appears that the distance at which two points could be felt as two on the back of the hand was 16.4 mm. and on the finger-tip 1.63 mm.; the former result being strikingly small as compared with Weber's tables.

The sensitiveness of the palm was tested by determining the minimum height from which the fall of a bit of card-board could be perceived. These bits of card-board weighed .9 mgr. and were cut in rectangles of 1 by 2 mm. from a sheet of millimeter paper pasted upon the card-board.

The apparatus used for testing the pressure sense was a modification of Fairbank's post-office balance in which the weights were placed upon the scale pan, thus exerting an upward pressure upon the finger resting upon a cushioned plate at the end of the beam. A comfortable and firm position was secured and an attachment provided by which fatigue was prevented. Two-sevenths of the weight on the scale pan acted upon the finger. The table records that additional weight (to the nearest 25 gr.) which could be correctly distinguished about 3 or 4 times from an initial weight of 500 gr. But few observations were taken and the result is only approximate. The general result is that a difference of about $\frac{1}{6}$ or $\frac{1}{7}$ of the initial weight may be correctly appreciated.

We also attempted to measure sensitiveness to pain. For this purpose we used a light hammer (weight 98.3 gr.) pivoted at a point 200 mm. from the center of its iron head, and allowed it to fall on the tip of the fore-finger of each hand. The back of the hand as well as the finger struck was supported. The table records the minimum number of degrees

through which the hammer must fall in order to cause a painful sensation. While this is naturally not a clearly defined point, still its constancy was surprising. The left hand appears to be more sensitive than the right. As few falls of the hammer as possible should be used in this test as the skin rapidly fatigues.

We take up next a description of the tests of vision. The printed page was first placed beyond the subject's vision, then gradually moved toward him along a sliding scale until he could just read it. The column of the table gives the distance at which, with the maximum strain, the page could be read. The size of the type is that in which this article is printed. The same page was then held as close to the eye as possible and yet have the subjet able to read it. We next record the smallest size of print (in dioptrics) that could be read at 25 feet.

For the next test we prepared a large white disc with small black sectors ranging from 1° to 15° and proceeding by half-degrees up to 5°. When this was rotated there appeared a series of concentric rings of various light shades of gray, each ring being 10 mm. wide and separated by 5 mm. from its neighbors. The subject counted as many concentric rings as he could see, and the result was then read off in degrees.

The acuteness of vision was tested in several ways, (A), by finding the distance at which a series of black lines 1 mm. wide and separated by spaces of 1 mm. could be recognized and the spaces between the lines clearly discerned, (B), by a similar determination with a checkerboard pattern, both black and white squares, being 4 mm. square, and (C) by the distance at which either 7 or 8, 11 or 12 and 15 or 16 dots 2 mm. in diameter and irregularly arranged in a rectangle of 25 x 40 mm., could be counted. The results are recorded in inches.

Our next test related to color and we attempted at the same time to detect any color defects, and to get some measurement of the rapidity and accuracy of color distinctions. Each student was required to match as rapidly as possible 30 colored ovals of a Magnus-Jeffries Color Chart (as published by Prang). We also noted irregularities in matching. The average time shows about six seconds for each color.

The strength of vision we tested by noting the smallest size of letter readable at 25 ft. through one and through two thicknesses of common cheese-cloth. No student could see the letters at all, up to 50 dioptrics through three thicknesses. The result is recorded in dioptrics.[1]

[1] The only test for hearing that we attempted was to determine from what height a shot weighing 10 mgmm. must be dropped upon a glass plate to have the sound heard by the subject at a distance of 25 ft. The

We also made a few tests of the rapidity of movement. This was done by arranging two keys so that the closure of the one would start a Hipp Chronoscope and of the other would stop it. The distance between the keys was in the one case 38 inches and in the other case 3 inches. With the keys 38 inches apart the subject was first told to touch them in succession, not as fast as possible but at any rate which seemed natural to him. He next made a movement of the same extent, as well as one of 3 inches, as fast as possible. This was done separately for the right and left hands, and the average time of about 5 movements is recorded in the table. The movement must be somewhat accurate in order that the key shall be struck at each end. The results for the maximum movements enables us to determine that the movement alone was at the rate of about 8 feet per second.

It had been our intention to meet each student a second time and with this intention we inaugurated a series of tests of sense-judgment, only a very small portion of which was completed, namely those relating to pressure and one relating to the space sense of the skin. The subject was first required to pour as much shot in the palm of his right hand as he thought would weight an ounce. The average weight of the shot thus estimated to weigh an ounce was 37 gm., or an exaggeration of 13% (men 47 gm., an exaggeration 65%; women 22 gm., an underestimation of 21%). He was next asked to pour as much shot into a box ($3\frac{1}{4}$x$3\frac{1}{4}$x4 in. made of $\frac{1}{8}$ in. pine) as he thought necessary to have shot and box weigh one ounce. In this case the average result was 97 gm. or an exaggeration of 242% (men 100 gm., exaggeration 252%; women 92.5 gm., exaggeration 226%). The illusion involved in this test is the well known fact that a stimulus spread over a larger area seems much less intense than a like stimulus confined to a more limited area. The result, in the two cases given above, measures the degree of the illusion. He next repeated the operation with the intention of making the box and shot weigh one pound. The average result was 548 gm. an exaggeration of 28% (men 605 gm., an exaggeration of 34%; women 463, an exaggeration of 2%). We find here a smaller percentage of exaggeration than in case of the ounce. He was then given the box which he regarded as one pound and irrespective of its actual weight was asked to put enough shot into another box to make it

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average result 27.8 mm. is inaccurate owing to the impossibily of securing absolute and constant quiet. It is interesting to note that the hearing of the women was more acute than that of the men, the results being 17 and 35 mm. respectively.

weigh double the first. The average result was 879 gr. or an underestimation of 20% (men 940 gr., underestimation 23%; women 789 gr., underestimation 15%).

The space-test consisted simply in spreading the points of the æsthesiometer on the back of the subject's hand until he regarded the distance between the points to be one inch. The average result was 30.6 mm., an exaggeration of 20% (men 31 mm., exaggeration 22%; women 30 mm., exaggeration 18%).

It is interesting to note that in all these tests of sense-judgment the women are more correct than the men.

In addition to this a few tests on bilateral symmetry of motion were made upon 17 of the lady students. They were asked to move the fore-fingers of the two hands outward from a common point along horizontal bars of a wooden cross the intention being to move the two arms to an equal distance. The movements were first made with the fingers at all points resting on the bar and were further subdivided into fast movements and slow movements, and again into large movements and small movements. All these variations were also gone through with for movements in which the fingers were lifted up into the air and brought down upon the bar at the end of the movement, (free movements). The table shows the result from each of these variations. It appears that, in each case, the right hand makes the larger movement, the excess on the average amounting to 15.5 mm. Regarding the extent of the excess of the *preferred* hand it is necessary to note that one student is markedly left-handed and another nearly ambidextrous. In both these cases the left hand makes the larger excursions and thus the average excess of the *preferred* hand becomes 16.7 mm. or $\frac{2}{3}$ of an inch.

It appears that the most influential of the distinctions made is that between the guided and the free movements, the average excess of the preferred hand in the case of the guided movements being 10.1 mm. and in free movements 23.4 mm. The size of the movement is of some influence upon this excess, it being on the average 21.3 mm. for the large movements and 12.1 mm. for small movements. In slow movement the excess of the preferred hand is more marked than in fast movements, being 19.9 mm. in the former and 13.5 in the latter. Individuals show considerable difference in the amount of this excess of the preferred hand, the average excess for the 17 different individuals being as follows: 54.3, 30.7, 30.1, 25.1, 22.6, 20.6, 17.6, 17.0, 12.8, 12.6, 10.9, 10.7 (left), 9.9. 9.0, 8.0, 8.0 (left), and 3.9 mm.

In addition to the measurements given above we placed before them a series of miscellaneous questions in regard to

personal and family characteristics. From the answers to these questions we collect the following data: the average age was 21 yrs. 11 mo. (31 men 22 yrs. 4 mo.; 22 women 21 yrs. 4 mo.) Of the 53 students 45 were born in Wisconsin, 7 in adjoining states while 1 is of foreign birth. Regarding the birth-place of the parents, in 29 cases it is in foreign lands, 23 in New England States, Vermont predominating, 32 in Middle States (N. Y. 28, Penn. 4), 21 from Western States.

The occupation of the father was noted with the following result: 15 merchants and manufacturers, 10 farmers, 13 professional men, 5 officials, 4 mechanics, 5 bankers and real-estate dealers.

When asked to state whether they regarded their health as "excellent," "good," "middling" or "poor," 20 (14 men and 6 women) pronounced it "excellent," 28 (13 men and 15 women) "good," 4 (3 men and 1 women) "middling" and 1 "poor." When questioned as to the existence of headaches or other chronic complaints 30 (16 men and 14 women) declared themselves free from all such, 13 (9 men and 4 women) were troubled with headache and 7 with other complaints.

46 of 52 students (27 men and 19 women) called their sleep "regular" and the rest "irregular," and 33 of 46 students (23 men and 10 women) spoke of their sleep as "sound," and the rest as "light." The average duration of sleep was just 8 hours.

It will be interesting to compare, as far as possible, the records of the men with those of the women. The general result regarding dermal sensations is that women have finer sensibility than men. This is true for each one of the tests made, but the differences are comparatively slight, except for the absolute sensitiveness of the palm and the sensitiveness to pain. The greater sensitiveness in women in both of these cases indicates freedom from rough usage.

As regards vision the differences on the whole are so small as to prove no superiority in the one case or in the other. To this there is but one exception and that is in the accuracy and rapidity of color perception in which the women are clearly better than the men.

Finally regarding the rate of movement, the normal movements, that is those adopted when no special direction is given, are quicker in women than men while the maximum movements, particularly in case of the longer movements, are faster in men. All these differences are consistently related to well recognized differences in the two sexes regarding the use and development of the different senses.

Table I.

Physical Measurements (of 31 Men, in mm.)

Height Standing.[1]	Height Sitting, from Seat of Chair.	Span of Arms.	Chest Girth.	Head Girth.	Strength of Squeeze.[2]
1748	926	1813	910	575	41.25

Table II.

Sensation and Movement.

Dermal Sensations.

Two Points Felt as Two.						Sensitiveness of Palm.			Pressure Sense.			Sensitiveness to Pain.					
Back of Hand.			Tip of Forefinger.									Right Hand.			Left Hand.		
T.[3]	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
16.4	17.5	15.0	1.63	1.71	1.52	44.0	58.2	21.9	82.5	83.7	80.7	26.7	33.9	16.6	19.3	22.7	14.8
(52)[4]	(30)	(22)	(54)	(32)	(22)	(49)	(27)	(22)	(53)	(31)	(22)	(53)	(31)	(22)	(52)	(30)	(22)

Sight (53 Students; 31 men, 22 women).

Distance at which print can be read.			Near point for print.			Smallest type visible at 25 ft.			Differentiation of white from gray.			Time for sorting 30 colors.			Distance at which lines can be recognized.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
52.9	53.5	52.1	2.5	2.4	2.7	8.3	9.4	6.7	2.62°	2.74°	2.42°	177″	212″	130″	108	117	97

Sight, continued, (53 students; 31 men, 22 women).

Distance at which dots can be counted.									Distance at which checker-board pattern can be recognized.			Letter visible through cloth.					
7 or 8.			11 or 12.			15 or 16.						1 thickness.			2 thicknesses.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
157	155	160	141	141	140	101	108	91	122	121	124	22.0	24.7	19.0	43.5	45.0	42.0

[1] The height of heel (average 21.2 mm.) has been subtracted from full height.

[2] This measurement was taken upon only 16 men and is expressed in kilograms.

[3] M is the result for the men, W that for women, T the average of both.

[4] The figures in parentheses give the number of persons tested.

Rate of Movement (45 students; 28 men, 17 women).

Movement through 33 inches.												Movement through 3 in.					
Normal.						Maximum.						Maximum.					
Right Hand.			Left Hand.			Right Hand.			Left Hand.			Right Hand.			Left Hand.		
T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.	T.	M.	W.
1000[1]	1070	885	908	964	817	542	506	601	527	498	574	181	181	181	185	172	205

[1] These numbers indicate $\sigma = .001$ sec.

TABLE III.

Symmetry Movements.

Guided.								Free.							
Fast.				Slow.				Fast.				Slow.			
Large.		Small.		Large.		Small.		Large.		Small.		Large.		Small.	
R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.
496	494	179	170	493	505	190	177	505	497	199	185	510	480	180	168

Addition to Literature Notices under articie on Zöllner's Illusion.

MÜLLER-LYER (Du Bois Reymond's Archiv. Supp. Band 1889) gives a brief but valuable account of a variety of optical illusions of judgment. He clearly demonstrates the influence of angles, of positions of figures, and the like upon their apparent size. His explanation of the illusions refers them to the tendency of considering surrounding and suggested areas in the judgment of lines and areas. He also mentions the effect of the angle in Zöllner's illusion, but does not enlarge upon its relation to the other illusions. The article, while comprehensive and original, does not add materially to the explanation of the illusion[1].

[1] The illusions of contrast in our article are described in Müller-Lyer's article. While I had read this article in 1889, I had entirely forgotten about it in the present investigation and worked out the present figures, which I had not seen before (they are not figured in Müller-Lyer's article but only incidentally described) independently. Dr. Sanford has drawn the figures described by Müller-Lyer, and through him my attention was again called to this figure and article *after* the present article was written.—J. J.

Corrections to "Studies from the Laboratory of Experimental Psychology of the University of Wisconsin." AM. JOURNAL OF PSYCHOLOGY. Vol. IV., No. 2.

On page 199 insert the following table, accidentally omitted:

RANGE OF WORDS.	J. J. σ.	F. W. σ.	MOTOR. J. J.	MOTOR. F. W.	SENSORY. J. J.	SENSORY. F. W.
Any word whatever	269	267	266	262	272	272
One of 100 verbs	260	265	253	263	265	267
One of 50 animals	250	262	250	256	250	268
One of 20 names	238	249	233	246	243	252
One of 20 letters	238	243	237	233	239	252
One of 10 French words . . .	245	251	246	249	244	253
One of 10 numbers	229	233	227	232	231	234
Simple Reaction Time	177	187	174	184	181	191

The pages in "Accessory Apparatus for Accurate Time-Measurements" belong to the study of "The Effect of Foreknowledge upon Repetition-Times," and the "Note upon Apparatus and Method" (p. 200) is a part of the former.

The "Note A—On the Timing of Rotating Discs," and the "Note on a device for color mixing "(p. 211) belong to the study of "A Novel Optical Illusion," and should be credited to Mr. Moorehouse. In the cut (p. 210) the letters B P on the right hand side should be B′ P′.

THE PSYCHOLOGICAL FOUNDATION OF NATURAL REALISM.

By Alexander Fraser, A. B.

The ordinary, common-sense man lives and thinks on the assumption of two fundamentally distinct and frequently conflicting worlds, the world of ideas and the world of things. The distinguishing characteristics of these two worlds are to him,—to put it in a word,—that the former may be and often is illusory, and that the latter must be and always is real, always the same permanent, unchangeable world. Throughout the greater part of his life the two coincide and present to him the appearance of only one, but occasionally there come critical moments at which they must part company and leave as a result of their conflict and separation a firm conviction of a real dualism; the world falls apart into two general classes, of which one must be real and the other may not. It is at such periods in the uncritical common-sense life, that it is easiest to observe the primordial germ of dualism, and the special psychological foundation of that belief in a real external world which is the presupposition of all practical life and the guiding-star of all realistic systems of philosophy. In the case of the unreflective but practical thinker, the question, what do you mean by a real world? is answered openly and without bias. The most general statement of his answer is: It is a world that we can *touch*. What is necessary according to him, in order to constitute the essential features of a real world, is that it be in some way or other *tangible*. What he means by the reality of an object seen in the distance is the belief that if he were beside it he could touch it; if upon approaching it he found that he could not feel anything, he would say that it was not real but illusory. What he means by an illusion, ghost or phantom, is, in an ultimate analysis, something which is in its very nature intangible. He can be persuaded that the object he *sees* before him is illusory; but if he is allowed to stretch forth his hand and can touch it and feel it there, the last remnant of doubt as to its real existence will have fled. Or conversely,

he can be persuaded that the ghost or phantom which he sees is really there, but if he puts out his hand and feels it not, then he is firmly convinced of the illusion. Practical life is full of illustrations of this truth, and I think that without making any further explanation, we can safely carry with us for future use the general conclusion that the final and most conclusive test of reality for the common-sense man is "touch."

But this truth can be seen in a much deeper and more critical sense. Let the common-sense man begin to philosophize. Let him become acquainted with Berkeley's theory of matter. He is told that this real world of his in which he has had all faith ever since his life began, is a monstrous illusion; that there is only one world and that that is not what he used to call his real world but his ideal world; that he is to be deprived of not one of his old facts, but that all these facts are of the same type and this type is the type of his ideal world. He is at once fascinated by the novelty of the doctrine. At first he will have an irresistible objection to it on the ground of his old appeal to reality—he will invariably reply, there is more than the *idea* of the world there, for I can *touch* it. But he is asked to *reflect*, to look within and to say what he really means by "touching an object," he is asked to describe, to give a definite expression to the content of consciousness which corresponds to this fact of touching; once more he begins to see the truth of idealism, and his stubborn realistic notions begin to fade and grow dim. He finds that all he means by "touching an object" is the *idea* that his hand (another idea) stands in a certain relation to an object, which is itself only an idea. Everything he attempts to describe or express must first be translated into this language of idealism. It is all very well for men to live uncritically and to *believe* in an external material world; it is all very well to say that we can touch it, but the true and ultimate test now is not "touch" but "expression." Describe the content of your consciousness. Try to express what you mean by matter, try to define it, and you will find it immediately dissolving into ideas. The whole belief in a material world has arisen from want of reflection, from want of the proper method for observing the truth of things. The way to get at the truth of things is not to believe what *is* here but to wait until the next moment and then look back and see what *was* there. At the moment when we touch an object we have an immediate belief in its real existence apart from our knowledge about it, but we must not have any faith in this belief—we must find the real truth about the object by reflecting on this belief and by trying to give it a definite expression. The arguments of the idealist

are unanswerable, and thus the common-sense man becomes a convert.

But let this same man arise from his philosophic calm, and let him once more go out and assume the duties of practical life; at the first stone he kicks, away goes Berkeley's theory of matter; he is back in his real world again. Idealism is very fascinating and all very true for a state of perfect calm, in which all the *active* senses are relaxed, but once out in the busy scenes of active life, its charms are gone, and all its terms appear hollow-sounding and meaningless. Underlying practical life there is a vast stretch of realistic intelligence which refuses to be expressed by the reflective method. It has no content in the imagination and consequently defies definite description. It seems to have been left without a language and without a written history. But nevertheless it has perhaps the highest claim to the name of intelligence since it realizes itself in immediate belief and practical life. Its outcome is not reflection but *action*. The real world we cannot and must not try to know by reflection, but we can and do know it by acting and living in it. How eagerly and yet how vainly do we search the whole vocabulary of language for words to express this great practical truth! How we have to fall back, as did the Scottish philosophers, on such generalities as "common sense," "belief," "intuition," which can be so easily ridiculed by the glib-tongued idealist whose rich inheritance is almost the whole vocabulary of thought! And what relief we feel in the reflection that we *are*, and *do*, more than we can know! Life and its fundamental beliefs are greater than knowledge; and the most fundamental belief, and the belief which stimulates and moulds all life, all evolution, all progress, is that belief which the ordinary man has in the existence of a real external world. It must be remembered that in this we are not dealing with any speculative form of realism. What we have been looking at is the simple experience of the naïve thinker. Our common-sense man has gone through the experience of idealism, and now he is back in his real world again. It is the same old world that once before he told us he could *touch*. It was by again allowing full play to the sense of touch that it was brought back to him with even deeper conviction than before. If we ask him now what is his criterion of reality he will reply not "definition" but "touch." He knows, and will admit, that there is something simple and uncritical about it, but yet he feels like crushing once for all our critical methods by telling us that there are more things in "touch" than were ever dreamt of in our philosophy. Such confessions from the ordinary unreflective life are of greatest importance inasmuch as they

point out to us the history of the belief in an external world in its first stages, and in this indicate the fundamental basis of realism and the true method for its solution. And now that this fact has been pointed to, that realism at least in its first conscious forms, that at least the primary stages of a belief in external reality are most directly connected with, if not wholly founded upon, the sense of touch, we can go back still farther and read from the story of evolution how all this came about.

Now why is it that touch should be the organ of reality any more than any other sense? *A priori* there is no reason. The only way we can realize and appreciate the fact is by observing its history. Touch is the mother-sense. It is a result of the first division of labor in aminal life. The division of the protoplasmic mass into endosarc and ectosarc, or tactual surface is the first sign that marks its individuality. The tactual surface is the primordial boundary line between the ego and non-ego. It is most closely allied with the vital functions. In many of the lower forms, such as the Amœba, the absorbing surface and the contact surface are co-extensive; the vital functions and the tactual functions are almost one—the hand, mouth and intestine, are one and the same organ. As differentiation goes on, the tactual surface makes its special duty more marked. It becomes more and more confined to the business of mediating between the inner life and the outer world. If an outer world is to have any relation to, if it is to communicate in any way with, if it is to have any meaning for the inner life and vital functions, it must do so by means of tactual impressions. All the other senses, as Spencer has pointed out, are only modifications of the sense of contact. In their rudimentary stages the space penetrating senses are nothing more than anticipatory forms of touch. Their primary office is to serve touch. If they are to have any meaning for the life of the organism their impressions must be translated into impressions of touch. In the most highly developed forms the primary use of these anticipatory senses seems to have been forgotten, and they are admired for what they are in themselves. In man, for example, the visual faculty instead of remaining exclusively in the service of touch as a special scout between the inner life and outer reality has also become connected with the business of imagination, speculation and hypothesis. But in so far as any of these senses give any intelligence of an essentially real world, they must serve in their primary capacity and translate their impressions into the original impressions of contact. The organism cannot be affected in any important, in any *real* way, except by actual contact. All intercommunications and

relations with an external world that are most closely connected with life are in their ultimate analysis relations of actual contact. Eating, breathing, locomotion, acquisition of food, struggles with and escape from enemies, all functions implied in the processes of life and evolution are functions which imply actual contact between the organism and its environment. Thus the sense of contact is that which is most closely allied with life on the inner side and with reality on the outer. It is the first and original *meaning* of reality. In the case of the other senses we may doubt and reason about the reality of the imformation received, but if we doubt the reality of contact we call in question the very standard by which we are enabled to doubt. And though in disease the sense of contact may deceive us and present to us illusions, yet the standard of sanity by which these phenomena are known to be illusions is the standard of contact.

Another fact which may be learned from the evolution of the sense of touch is the history of that *immediate belief* and *prompt reaction* which always accompanies it. In the case of the space-penetrating senses there is no absolute necessity for immediate belief in and prompt reaction to the information received. At the sight of the enemy in the distance it is not absolutely necessary that the animal should immediately take the proper measures for warding off the attack. It has plenty of time to stop and speculate as to whether it is a real enemy, admire its form, etc., and still have time left to make itself secure from danger. The anticipatory faculties are only the first warnings of approaching interests and may be and quite often are illusory and misleading. The characteristics of the reaction which follows must consequently be wavering, hesitation, delay, and speculation. But the case of contact is very different. By touch the final warning is given, and if it is not heeded and immediately reacted to, destruction or injury is sure to follow. There is no time for reflection, doubt, or speculation. It is the final signal and the animal which is not so constituted as to follow it with immediate belief in its reality and prompt reaction, will not survive in a real world. One of the fundamental conditions then, on which the sense of contact has survived as the special organ of a real external world, was that its outcome should consist in immediate belief and prompt reaction, and for this reason it is so to-day. At the beginning of the history of animal life, its sole function was to mediate directly between the inner life and the external world; this is its special function to-day in the latest stages of the history and in its most developed forms.

It may at first sight be thought childish to form all this real world of ours in all its fulness and vast complexity on

such a simple, crude and seemingly unconscious thing as the sense of touch. Touch, however, is far from being crude. The mother sense, if it has not kept ahead, has at least kept abreast in development with the others. The influence of the sense of contact can be traced in all the highest forms of intelligence. Herbert Spencer says that touch is "more than any other sense associated with the advance of intelligence." He finds from the facts of evolution that "a highly-elaborated tactual apparatus comes to be the uniform accompaniment of superior intelligence." In support of this he supplies facts from each great division of the animal kingdom. The *Cephalopoda*, the most sagacious of the *Mollusca*, are especially distinguished in structure in having several arms by which they can grasp an object on all sides at the same time that they apply it to the mouth. Again the crabs which stand at the head of the sub-kingdom *Articulata*, bring their claws and foot-jaws simultaneously to bear on things they are manipulating. The parrot, which of all birds is admitted to be the most intellectual, differs most from its kindred in the development of its tactual organs. No other bird approaches it in the complexity of the tactual actions it performs and the tactual impressions it receives. Among mammals the *Unguiculata* or those having limbs terminating in separate digits are more intelligent than the *Ungulata* or hoofed animals. The feline and canine tribes stand psychologically higher than cattle, horses, sheep and deer. In the case of any marks of sagacity among hoofed animals, as in the horse, the lack of sensitive extremities is partly compensated by highly sensitive and mobile lips. The most remarkable and most conclusive instance of this connection between the growth of intelligence and development of the tactual organs is seen in the elephant, which is markedly distinguished from allied tribes both by its proboscis and by its great sagacity. The association between intelligence and tactual powers is brought out more conspicuously in this case by the fact that both are exceptional. Among the *Primates* the same association of development of intelligence with that of tactual appendages is distinctly marked both in contrasts between them and inferior animals, and between the different genera of themselves. The prehensile and manipulatory powers of the lower kinds are as inferior as their mental powers. In the case of the human being, Mr. Spencer maintains not only "that the tangible attributes of things have been rendered completely cognizable by the complex and versatile adjustments of the human hands, and that the accompanying manipulative powers have made possible those populous societies in which alone a wide intelligence can be evolved" but that even "the most far-reaching

cognitions, and inferences the most remote from perception, have their roots in the definitely-combined impressions which the human hands can receive."

Again, it may be objected that the sense of contact as such is only a myth; that what we have been calling the mother-sense is only a name or hypothetical term introduced for the purpose of explaining the origin and differentiation of the other senses, and that there is no such thing as a definite and special sense of contact. The evidence from experimental psychology, as far as it has gone, goes to show that this objection is without good foundation. The sense of touch is perhaps of all senses the least explored, but the bulk of facts already obtained by experiment give evidence that apart from the variety of sensations generally grouped under the word "touch," *i. e.* the feelings of pain, exertion, fatigue, conæsthesia and muscle sense, there is a *special* sense of *contact*. Goldscheider by drawing a very fine point of metal over the skin discovered that at certain minute points a distinct and peculiar sensation of "pressure" was felt. This sensation, when the pressure is very light, is described as being lively and delicate and accompanied by the feeling of being tickled. When the pressure is increased, the character of the sensation changes and becomes as though a small, hard kernel were pressed upon the skin. Stimulation of the spaces between these spots does not produce the same characteristic sensation but rather a dull, indefinable, "contentless" sensation.[1] This special sense of pressure or of tactual hardness is incommensurable with any of the accompanying sensations. It cannot be explained by any possible combinations of any other senses such as the feeling of innervation, muscular resistance, etc., but it is in itself something unique and underived. Is it not the sense which alone gives us the essential nature of the primary qualities of matter? The feeling of muscular resistance has a meaning, but it is a very different meaning from that of resistance plus contact. Muscular resistance can never get beyond a muscular feeling—it can never mean hardness, solidity or those fundamental strata of matter which we call the primary qualities. Landry gives the case of a workman whose fingers and hands were insensible to all contact but in whom the sense of muscular activity was everywhere alert. His eyes were shut and a large object placed in his hand. He was quite aware of the muscular resistance but had not the slightest notion of an object, or that an object was in his hand; his only idea was

[1] GOLDSCHEIDER, *Neue Thatsachen über die Hautsinnesnerven*, Du Bois-Reymond's Archiv, 1885, Suppl.-Band, 76.

that he could not close his hand, and he was astonished at the fact. Such facts as these, then, obtained by experiment, tend to show that there is a special sense of contact which is distinct from, and incommensurable with, the other senses; that this sense is the special organ for cognizing the primary qualities of the material world; and that consequently the mother-sense is not a myth or hypothetical name, but a real specific sense.

Now that we have seen the historical foundation for the important part which the sense of touch plays in the practical knowledge of common sense, we can go still farther and trace its influence on the more technical forms of intelligence, science and philosophy. "All developed science," says Mr. Spencer, "dealing as it does with *measured* results, is lineally descended from that simplest kind of measurement achieved by placing side by side the bodies held in the hands. Our knowledge of the forces governing the Solar System is expressed in terms that are reducible, by an ultimate analysis, to equal units of linear extension, which were originally fixed by the direct apposition of natural objects. And the undeveloped sciences that have not yet passed the stage of qualitative prevision, depending for their advance, as they do, either on experiments requiring skillful manipulation or on observations implying dissection and other analogous procedures, could not have reached this stage in the absence of a highly developed manual dexterity." Science is not only mechanically dependent on the sense of touch but it is so in its very nature. The very world that science is striving to express is the world of contact. It never rests satisfied until it can define things in terms of the tangible. Contact is the presupposition of all scientific investigation. All psychological theories, for example, take for their starting point the conception of contact. The various empiricist theories of the development of the notion of space all begin with "contact." All theories concerning the processes involved in the functions of the various senses are attempts to reduce these processes to terms of contact. Sight is not explained by sight but by a hypothetical process instituted in order to allow actual contact between the retina and the object. In the same manner also are hearing and smell explained. Again all physical theories presuppose this same conception. All physical hypotheses about atoms, fluids, vibrations, etc., are just the outcome of this attempt to give expression to this fundamental and unnameable yearning after tactual terms. It is a mistake to say that the goal of science is the "continuum"—the paradoxical and inconceivable continuum. The continuum is really not a concep-

tion at all, it is merely a name applied to that feeling of vain and endless effort, that contradiction which we feel when we try to express or describe the conception of contact in visual terms. It is merely the term applied to the contradiction which arises from trying to exhaustively describe the original notion of contact by means of modifications of the notion of visible expanse. The various hypotheses of atoms, fluids, etc., are not true expressions of the notion of contact; they are really visual constructions of the imagination and are in their very nature incapable of defining it. They serve very well as arbitrary *signs* of this notion but when they are looked upon as anything more they are bound to lead to contradictions. But the important point to be observed is that the goal of the existence of such hypotheses, the one fundamental purpose for which they are constructed is to make contact possible. Thus the underlying presupposition of science is not the "continuum," but "tangibility." A tangible world is the kind of world it is striving to express. All things can be made clear, can be scientifically explained if they can be reduced to the type of the tangible.

In the sphere of philosophy the influence of the touch-world is not so apparent, and its importance is much less frequently asserted. It seems to be swamped, as was indicated before, by the character of the philosophic method. The tangible world cannot flourish on introspective and reflective soil. The introspective type of reflection which to such a great extent characterizes the current methods of philosophy, seems for the most part to be the visual type of knowledge, and stands just as incapable of describing the phenomena of touch as that of sound or any other sense. This visual type of knowledge recognizes the existence of the tangible world in the sense that it believes that there is a real world to express. But when it formulates a visual expression of this world, it begins to see that its visual lines have fallen in unpleasant places and present nothing more than a mass of abstractions and contradictions, such as the "continuum," "abstract substance," and all the other bug-bears of philosophy. This mass of absurdities it surely must discard; and mark just here how scepticism follows. Instead of calling in question its method of expression and seeing its inadequacy, it regards this so called "mass of absurdities" as a true expression of the real world, and consequently resorts to the conclusion that there is no real world at all—the real world having become identified with this chaotic expression is rejected with it. This method of philosophy is the foundation of the Berkeleyan type of idealism and the scepticism of Hume. The material world

to which Berkeley meant to deny existence was not the world of touch but the chaotic offspring of the visual expression of that world, and in this he took a very important step towards clearing away the "philosophic dust;" but immediately afterwards he took a seriously false step in attributing the fault in this inadequate expression to the side of the touch-world rather than to the method of expression. In consequence of this he led the way to the denial that there was any real world to express, and this false step is carried out and fully developed with all its implications in the sceptical philosophy of Hume.

The thoroughgoing criticism of Hume marks the period for the beginning of a new system of philosophy. Hume boldly encountered the great paradox involved in the attempt to express the real world by the reflective method, accepted it as unavoidable, and denied the possibility of metaphysics. Now, if there is to be a new positive philosophy, this paradox must be solved, and this is possible, obviously, only on condition of a change in the philosophic method. In making this change there are two alternatives: either the reflective method must be retained and greatly modified and manipulated, or it must be abandoned altogether and the external world must be asserted from the side of its own special sense, which throughout this paper we have been trying to maintain is the sense of touch. Has philosophy ever attempted this? The tangible world we saw, forcibly asserts its influence and importance throughout the earliest stages of animal life, in the practical world of common sense, and in the domain of science. Now the question is, has it done so also in philosophy, or has it in this sphere been altogether neglected? Is there any evidence that there is any one system of philosophy whose characteristic method of procedure, whose characteristic type of thought we can identify with the type of touch?

We shall try to adduce evidence to show that what may be called the psychological foundation of the Scottish school of philosophy, Natural Realism, is the sense of touch; that the particular type of thought, or thought-temperament which is the underlying possibility of such a doctrine is the "touch type." Or to be more particular, what we shall try to prove is that the real external world which this school of philosophy so bravely defends, and tries so hard to express, is not a world known by some inexplicable divine intuitive act of consciousness as they thought, but the simple and hitherto unattended to, phenomena of the *special* sense of touch; and that the characteristic "immediate" type of knowledge by which they conceived this world to be known, can be identified with those processes which are peculiar to tactual perception.

The essential point to be noted in the doctrine of Natural Realism is that it is a reaction against the Lockian "theory of ideas." According to this theory of ideas all knowledge is mediate, we can only know things through their ideas. Now the school of Realism, noticing the sceptical outcome of this doctrine, reviews it, and finds that, though there is a great deal of truth in it, yet it is only a partial view. Realism says that "mediate" knowledge is not all; there is immediate knowledge; there is a certain kind of knowledge in which there is no *tertium quid*. Or again, Realism may be said to be a forcible return to perception. The Berkeleyan idealism reduced perception to the type of conception. Realism brings perception back to its original type and emphasizes it. The watch-word of the whole system is "immediate perception." And now that we have the doctrine as it were in a nut-shell, all we have to do is to find out what is really meant by "immediate perception"—what is the type of knowledge it expresses. In order to do this let us first see what sort of criticism the Scottish philosophers passed on the reflective method of idealism, and what method they proposed to put in its place.

The criticism they passed on the method of reflection was essentially psychological. They looked into the psychological basis of the method. And what do they find? That the whole system is built up on an analogy of visual processes. They analyze the language of philosophy and they find that it is made up almost wholly of visual terms.

Dugald Stuart says:

"Another observation too, which was formerly hinted at, is confirmed by the same historical review; that in the order of inquiry, the phenomena of vision had first engaged the attention of philosophers, and had suggested to them the greater part of their language, with respect to perception in general; and that in consequence of this circumstance, the common modes of expression on the subject, unphilosophical and fanciful at best, even when applied to the sense of seeing, are in the case of all the other senses obviously unintelligible and self-contradictory."

Dr. Thomas Reid gives the same criticism of the so called idealistic method:

"Of all analogies between the operations of body and those of the mind, there is none so strong and so obvious to all mankind as that which there is between painting or other plastic arts, and the power of conceiving objects in the mind. Hence, in all languages the words by which this power of the mind and its various modifications are expressed, are analogical and borrowed from those arts. We consider this power of the mind as a plastic power, by which we form to ourselves images of the objects of thought."

"In vain should we attempt to avoid this analogical language, for we have no other language upon the subject; yet it is dangerous and apt to

mislead. All analogical and figurative words have a double meaning, and if we are not very much upon our guard, we slide insensibly from the borrowed and figurative meaning into the primative. We are prone to carry the parallel between the things compared farther than it will hold, and thus very naturally to fall into error."

The idealistic method of philosophy then, both Reid and Stuart recognize to be essentially of the visual type. All the current philosophical language is saturated with visual terms and becomes perfectly unintelligible when employed to express the phenomena of the other senses. Natural Realism has a great truth to express but it can find no language that will express it—the visual language of philosophy will grossly misrepresent it. This is the general criticism. But there is one central point in which this visual method shows its inadequacy to express the truth of Realism, and in this we can make the first step towards the psychological interpretation of what is meant by immediate perception. The place where idealism and realism part company once for all is in the distinction between the primary and secondary qualities of matter. Idealism makes no absolute distinction between them, and allows both alike to be expressed by its "ideas" or "visual images." The point upon which Realism insists is that there is something in the nature of the primary qualities that absolutely refuses to be expressed by the same method that expresses the nature of the secondary ones. This peculiarity is "the direct" and "distinct notion" which we get "of what they are in themselves." Dr. Reid expresses the distinction thus:

"Is there anything common to the primary which belongs not to the secondary? And what is it?

"I answer, that there appears to me to be a real foundation for the distinction; and it is this—that our senses give us a direct and a distinct notion of the primary qualities and inform us *what they are in themselves*. But of the secondary qualities our senses give only a relative and obscure notion. They inform us only, that they are qualities that effect us in a certain manner—that is, produce in us a certain sensation; but as to what they are in themselves, our senses leave us in the dark."—Reid's Collected Writings, edited by Hamilton. (seventh edition) Vol. I, p. 313.

This "direct notion of what things are in themselves" is what Reid means by immediate perception, as all who are acquainted with his philosophy will know; and from the above passage we learn that it is the peculiar type of knowledge by which we know the primary qualities as distinct from the secondary. This kind of knowledge, he maintains, cannot be reduced to the mediate type; it is a type which must be expressed after its own peculiar fashion. It can be seen from the following quotation from Reid that what is really meant here is tactual perception. Speaking of the difference between visible and tangible magnitude he says:

"Such differences in their properties led Bishop Berkeley to think that visible and tangible magnitude and figure are things totally different and dissimilar, and cannot belong to the same object.

"And upon this dissimilitude is grounded one of the strongest arguments by which his system is supported. For it may be said, if there be external objects which have a real extension and figure, it must be either tangible extension and figure, or visible, or both. The first appears absurd; nor was it ever maintained by any man that the same object has two kinds of extension and figure totally dissimilar. There is then only one of the two really in the object, and the other must be ideal. But no reason can be assigned why the perceptions of one sense should be real, while those of another are only ideal; and he who is persuaded that the objects of sight are ideas only, has equal reason to believe so of the objects of touch.

"This argument, however, loses all its force, if it be true, as was formerly hinted, that visible figure and extension are only a partial conception, and the tangible figure and extension a more complete conception of that figure and extension which is really in the object."—*Essays on the Intellectual Powers of Man*, Collected Writings, I, 325.

In this passage the psychological interpretation of Reid's conflict with Berkeley is made very clear. Both agree that the visual and the tangible worlds are incommensurable as such, yet both want to give the world a homogeneous expression. In doing this they part company; Berkeley takes the visual world and makes the tangible conform to its type; Reid prefers the tangible and makes all conform to its type.

But we can make a more special analysis of what Natural Realism means by the intuitive conception of external reality. Dr. Reid distinguishes carefully between what he calls his "conception" of hardness and the "sensation" which accompanies the touching of a body.

"Let a man press his hand against a hard body, and let him attend to the sensation he feels, excluding from his thought everything external, even the body that is the cause of his feeling. This abstraction indeed is difficult, and seems to have been little, if at all, practised. But it is not impossible, and it is evidently the only way to understand the nature of the sensation. A due attention to this sensation will satisfy him that it is no more like hardness in a body than the sensation of sound is like vibration in the sounding body.

"I know of no ideas but my conceptions; and my ideas of hardness in a body is the conception of such a cohesion of its parts as requires great force to displace them. I have both the conception and belief of this quality in the body, at the same time that I have the sensation of pain by pressing my hand against it. The sensation and perception are closely conjoined by my constitution, but I am sure they have no similitude; I know no reason why one should be called the idea of the other, which does not lead us to call every natural effect the idea of its cause."—Reid's Collected Writings, edited by Hamilton, Vol. I, p. 317.

He presses his hand against a hard body, he feels certain sensations in his hand, temperature feelings, muscular feelings, feelings of fatigue, feelings of one part pressing against another, all of which he recognizes as some affection of his

hand and which he is pleased to call by the name of "sensation." But none of these gives him the "conception" of hardness; what he means by hardness is something very different and foreign to all of them. Yet accompanying these "sensations" he gets this "conception" of hardness; he gets it at the same time that he gets the subjective feeling in his hand; he knows not how, he simply gets it. Now does not all this look very much as if that which Reid called "conception" of hardness was just the *special* sensation of touch? He did not know that there was such a thing as a special sense of touch distinct from those other feelings which appear as affections of the skin, and what is more likely than that he should christen the feeling which he got from it by such a name as "intuitive conception?" But all this will be made clearer and more conclusive by the following passage:

"There is, no doubt, a sensation by which we perceive a body to be hard or soft. This sensation of hardness may easily be had, by pressing one's hand against the table, and attending to the feeling that ensues, setting aside, as much as possible, all thought of the table and its qualities, or of any external thing. . . . .

"There are, indeed, some cases, wherein it is no difficult matter to attend to the sensation occasioned by the hardness of a body; for instance, when it is so violent as to occasion considerable pain: then nature calls upon us to attend to it, and then we acknowledge that it is a mere sensation, and can only be in a sentient being. If a man runs his head with violence against a pillar, I appeal to him whether the pain he feels resembles the hardness of the stone, or if he can conceive anything like what he feels to be in an inanimate piece of matter.

"The attention of the mind is here entirely turned towards the painful feeling; and, to speak in the common language of mankind, he feels nothing in the stone, but feels a violent pain in the head. It is quite otherwise when he leans his head gently against the pillar; for then he will tell you that he feels nothing in his head, but feels hardness in the stone. Hath he not a sensation in this case as well as in the other? Undoubtedly he hath; but it is a sensation which nature intended only as a sign of something in the stone; and, accordingly, he instantly fixes his attention upon the thing signified; and cannot without great difficulty, attend so much to the sensation as to be persuaded that there is any such thing distinct from the hardness it signifies.

"But, however difficult it may be to attend to this fugitive sensation, to stop its rapid progress, and to disjoin it from the external quality of hardness, in whose shadow it is apt immediately to hide itself; this is what a philosopher by pains and practice must attain, otherwise it will be impossible for him to reason justly on this subject, of even to understand what is here advanced. For the last appeal, in subjects of this nature, must be to what a man feels and perceives in his own mind.

"It is indeed strange that a sensation which we have every time we feel a body hard, and which, consequently, we can command as often and continue as long as we please, a sensation as distinct and as determinate as any other, should yet be so much unknown as never to have been made an object of thought and reflection, nor to have been honored with a name in any language; that philosophers, as well as the vulgar, should have entirely overlooked it, or confounded it with that quality of bodies which we call *hardness*, to which it hath not the least similitude. . . . .

"The firm cohesion of the parts of a body, is no more like that sensation by which I perceive it to be hard, than the vibration of a sonorous body is like the sound I hear: nor can I possibly perceive, by my reason, any connection between the one and the other. No man can give a reason, why the vibration of a body might not have given the sensation of smelling, and the effluvia of bodies affected our hearing, if it had so pleased our Maker. In like manner, no man can give a reason why the sensations of smell, or taste, or sound, might not have indicated hardness, as well as that sensation which, by our constitution, does indicate it. Indeed, no man can conceive any sensation to resemble any known quality of bodies. Nor can any man show, by any good argument, that all our sensations might not have been as they are, though no body, nor quality of body, had ever existed.

"Here, then, is a phenomenon of human nature, which comes to be resolved. Hardness of bodies is a thing that we conceive as distinctly, and believe as firmly, as anything in nature. We have no way of coming at this conception and belief, but by means of a certain sensation of touch, to which hardness hath not the least similitude; nor can we, by any rules of reasoning, infer the one from the other. The question is: How we come by this conception and belief?

"First, as to the conception: Shall we call it an idea of sensation, or of reflection? The last will not be affirmed; and as little can the first, unless we will call that an idea of sensation which hath no resemblance to any sensation. So that the origin of this idea of hardness, one of the most common and most distinct we have, is not to be found in all our systems of the mind: not even in those which have so copiously endeavoured to deduce all our notions from sensation and reflection.

"But, secondly, supposing we have got the conception of hardness, how came we by the belief of it? Is it self-evident from comparing the ideas, that such a sensation could not be felt, unless such a quality of bodies existed? No. Can it be proved by probable or certain arguments? No; it cannot. Have we got this belief, then, by tradition, by education, or by experience? No; it is not got in any of these ways. Shall we then throw off this belief as having no foundation in reason? Alas! it is not in our power; it triumphs over reason, and laughs at all the arguments of a philosopher. Even the author of the "Treatise of Human Nature," though he saw no reason for this belief, but many against it, could hardly conquer it in his speculative and solitary moments; at other times, he fairly yielded to it, and confesses that he found himself under a necessity to do so.

"What shall we say then of this conception, and this belief, which are so unaccountable and untractable? I see nothing left, but to conclude, that, by an original principle of our constitution, a certain sensation of touch both suggests to the mind the conception of hardness, and creates the belief of it." . . . .

"What hath been said of hardness, is so easily applicable, not only to its opposite, softness, but likewise to roughness and smoothness, to figure and motion, that we may be excused from making the application, which would only be a repetition of what hath been said. All these, by means of certain corresponding sensations of touch, are presented to the mind as real external qualities; the conception and the belief of them are invariably connected with the corresponding sensations, by an original principle of human nature."—*Inquiry into the Human Mind*, Collected Writings, Vol. I, p. 120.

In this passage there are noticeable two special points in which there is a most striking resemblance between Reid's "intuition of the primary qualities" and the special sense of contact. 1. In order to get the sensation of contact proper,

there must be a certain amount of pressure on the skin. If the pressure is very light we get the "tickle" sensation. On increasing the pressure within certain limits we get the sensation of contact proper—the feeling which is of the nature of the "hard kernel." On increasing the pressure still further we get the more subjective type of feelings, muscle-sense, pressure of the muscles against one another, tendon sensations and perhaps "innervation feelings," pain, etc. Now Reid, in this passage, is very careful to make plain that his "intuition" only accompanies that degree of pressure which is within the limits of the sense of contact. Can we desire any more conclusive circumstantial evidence that this indefinable "conception" or "intuition" is just the specific sense of touch? 2. The characteristic which most clearly distinguishes the sense of touch from all other senses, is that it is the final and ultimate appeal to reality. The reality which we get by all other senses has the characteristic of being *inferential*—we always can reason as to the real existence of what they inform us about—we can doubt its reality and often have reason to—but the reality of touch is ultimate; we can have no proof of it; it is its own proof, its reality is given immediately. The interest of touch is always practical, and never speculative. There is no separation of the sensation from the belief. How all this came about according to the principles of natural selection, we saw before. Now this is just the character of Reid's "intuition" of the real world. Reason or reflective thought may deny it, may ignore it, in its philosophical seclusion from active life, but in real life, in practical life, it laughs at reason. This "belief" type of knowledge, which was before shown to be the special character of the "touch" type, is the characteristic which distinguishes Natural Realism as a distinct system of philosophy; this can be seen from many such passages as the following:

"We know what rests on reason, but believe what rests on authority. But reason itself must at last rest on authority, for the original data of reason do not rest on reason, but are necessarily accepted by reason on the authority of what is beyond itself. These data are therefore, in rigid propriety, Beliefs or Trusts. Thus it is that in the last resort we must perforce philosophically admit that belief is the primary condition of reason, and not reason the ultinate ground of belief." "The ultimate facts of consciousness are given less in the form of cognitions than of beliefs. Consciousness in its last analysis—in other words, our primary experience—is a faith. We do not in propriety *know* that what we are compelled to receive as not-self is not a perception of self; we can only on reflection *believe* each to be the case *in reliance on the original necessity of so believers imposed on us by nature*."—HAMILTON, *Discussions*, p. 86.

Sir William Hamilton agrees with the fundamental principle of the doctrine as laid down by Reid and Stuart. He re-

tains the doctrine of immediate perception but with some modification. The change which he makes, stated in a word, consists in narrowing down the amount of non-ego or external reality perceived, and the particular way in which he does this makes the evidence all the stronger that the "immediate perception" is in its ultimate analysis, the sense of contact. The tendency of Reid in his uncritical enthusiasm over his great truth, was to regard the immediate type of knowledge as extending over a very large area of thought, but Hamilton is more critical and makes an effort to find out its original meaning and to what particular sphere it belongs. To what extent do we have this intuitive perception of external reality, he asks, and what are the sole conditions on which it is possible? He discovers, in answer to this, that the object of perception, in so far as it is a quality of the extra-bodily world, is that which is in *contact* with the organ of sense. "An external object is only perceived inasmuch as it is in relation to our sense, and it is only in relation to our sense inasmuch as it is present to it." The only way any *real* external thing can affect us, is by actual contact. The only terms in which *reality* can express itself as such, are terms of contact. A few quotations will make this clear.

"We perceive through no sense aught external, but what is in immediate relation and in immediate *contact* with its organ; and that is true which Democritus of old asserted, that all our senses are only modifications of touch. Through the eye we perceive nothing but the rays of light in relation to, and in contact with the retina; what we add to this perception must not be taken into account."—*Metaphysics*, Lecture XXV.

"To say that we perceive the sun and moon is a false or elliptical expression. We perceive nothing but certain modifications of light in immediate relation to our organ of vision. It is not by perception, but by a process of reasoning, that we connect the objects of sense with existence beyond the sphere of immediate knowledge. It is enough that perception affords us the knowledge of the non-ego *at the point of sense.* To arrogate to it the power of immediately informing us of external things which are only the causes of the object we immediately perceive, is either positively erroneous or a confusion of language."—*Metaphysics*, Lecture, XXVII.

Is this not a strong point then, in favor of the position we are trying to support, that the most acute representative of the doctrine of Natural Realism, should find the only possibility of a direct knowledge of external reality, in the one sense of contact?

Very closely allied to the sense of contact is the muscle sense. We always find the most highly developed tactual appendages also the most mobile, and it is quite true that locomotor sensations play a great part as concomitants to the sense of touch in making up our knowledge of the external

world. But touch nevertheless gives us the *essential* feature of our world; it supplies the content, as was indicted before by the experiment of Landry. The muscle sense may help us to say that there is an external world, but it is the sense of contact that says what world it is. Considering this close alliance and co-partnership between the two senses, we might expect a tendency on the part of Natural Realism to explain the notion of external reality on the basis of the sense of resistance to effort. And this is just what we find. Hamilton in his later writings drifts towards this idea. Yet, in his case, it is quite evident from his doctrine as a whole, that what he really means is resistance *plus contact*. Just in so far as he would mean simple muscular resistance without the sense of contact he would not be a Natural Realist, as is well seen in the following criticism by Professor Veitch, a typical Natural Realist, on this very tendency of Hamilton's later thoughts:

"It seems doubtful whether the apprehension of resistance or of a resisting something as extra-organic in the locomotive effort is fitted or sufficient to give the intuition of extension or an extended thing. The intuition of resistance might be quite well satisfied by a force—a degree or intension of force—in correlation with the organism. Electricity would be sufficient to impede the locomotive effort; yet we should hardly regard this as adequate to give us the intuition of an extended object, though it might be apprehended as external. These considerations tend to show that the locomotive power has received somewhat exaggerated importance as a factor in our apprehension of extra-organic objects. The three sources of knowledge—Contact, Pressure, and Locomotion—seem to me to be required to go together, and yield a conjoint result, ere we can form the complex notion of body,—as external, extended, and resisting."—VEITCH, *Hamilton*, Blackwood's Philosophical Classics, 141, Glasgow 1888.

The external world for which the Scottish philosophers are contending, then, is not a world that can be inferred from muscular resistance; it may be known in connection with this resistance but is not derivable from it. This point is brought out even more emphatically in the criticism of Natural Realism proper on the doctrine of Inferential Realism as given by Dr. Thomas Brown. The pith of Brown's doctrine can be seen from the following quotation:

"To what, then, are we to ascribe the belief of external reality which now accompanies our sensations of touch? It appears to me to depend on the feeling of resistance, which, breaking in without any known cause of difference on an accustomed series of feelings, and combining with the notion of extension, and consequently of divisibility, previously acquired, furnishes the elements of that compound notion which we term the notion of matter. Extension and resistance—to combine these simple notions *in something which is not ourselves*, and to have the notion of matter, are precisely the same thing."—L. XXIV., p. 150.

The following is the criticism of Natural Realism as given by Professor Veitch:

"This is a singular and glaring specimen of *petitio principii*. Whence our belief in external or non-mental existence? Extension and resistance are "feelings," "notions," subjective states merely. These combined can but constitute a more complex mental state. This is not an external reality,—it is not the matter Brown is in search of. But he quietly adds, "to combine these simple notions in something which is not ourselves, and to have the notion of matter, are precisely the same thing." But when and how do we get this "something which is not ourselves," this "something" which is over and above our sensations? This is not explained; it is assumed. . . . . But Brown's inference of a cause of resistance in something that is not self, is wholly unwarranted on the premises and by the process here given. (1) It is supposed to be reached on the principle, assumed to be intuitive, of similar antecedents having similar consequents. When antecedents are similar, consequents are similar; true, but for all this there may be events which have no antecedents at all in the case, it will be in virtue, first of all, of the principle that every event or change in our experience has a cause—a cause of some sort. This principle or necessity is not involved in the principle, that where antecedents are similar, consequents are similar; on the contrary, this latter principle is founded on the other as one at least of its essential elements. (2). But if we carry out our inference on the principle of difference of antecedent from difference of consequent, the antecedent inferred will still necessarily be one within our experience, not a something wholly unknown to us, of which we cannot predict either affirmatively or negatively. I have the feeling of resistance; I know nothing more; I have no right to speak of "some object opposed to me." This is to introduce an object which is not a sensation. But why speak here of an antecedent at all? There is even no antecedent in time here. The feeling of resistance is not, ex hypothesi, preceded in my states of consciousness by anything I know, or any state of consciousness. It arises suddenly, unexpectedly, from nothing known to me that has gone before. I have no known antecedent to fall back upon; and as my whole knowledge or consciousness in the matter is limited to antecedents which are states of my own mind, I ought naturally to seek the antecedent among these, not in the wholly new notion of something opposed to me,—some object which is not myself,—an object which transcends alike my experience and my knowledge. If I do reach this notion, I certainly do not get it by the principle of the similarity of sequence between antecedents and consequents. And just as little can I reach it by the principle of casuality. This principle might tell me there is *a* cause of the feeling of resistance; it could never tell me *what* that cause is, or give to me the new notion of a particular cause. . . . . Any form of cause—spiritual or material alike—satisfies the idea of cause. How then can I thus account for this belief in corporeal substance distinct from myself. Obviously, the whole process is a mere fallacy. And if we have this belief which Brown assumes, it never arose in the way he supposes it did. We have no alternative but to retrace our steps, and to admit with Hamilton that we have illegitimately rendered the immediate perception or intuition of the external object from the irresistable belief in it; that, in fact, we believe in an outward world in space because we know an outward world there, and believe that we know it."—VEITCH, *Hamilton*, Blackwoods Philosophical Classics, 168, Glasgow 1888.

Let us stop here with the consideration of the more particular points of evidence, and let us look at the whole matter from a general standpoint. From the main characteristics or general symptoms of the doctrine of Natural Realism, what is it that we find? Here is a peculiar doctrine of the perception of the external world—the statement of a peculiar type of perception. The essential feature of the world known by this kind of perception, is that it is not deducible or derivable from anything that we know; it is not derivable from the feeling of muscular movement, resistance to muscular movement, nor from any possible combination of any of the sensations which accompany it; it is known *directly* and in itself, its own nature is the only thing that can define it. Now of what are these the symptoms? Psychology only knows of one thing in psychic life which presents these same marks, and that is what it calls the "special sense." The special phenomena of the sense of sight though scientifically *explained* by the notion of waves of ether, can never be deduced from such a notion, or known in any other way except by means of "seeing." Does not this peculiar kind of perception of Natural Realism then, look very much like perception by some special sense? Supposing that we take for granted that it is so, is there any further evidence on the matter? Is there any evidence which goes towards defining what that special sense is? From the passages already quoted it is evident that the sphere to which this realistic perception originally belongs is the knowledge of the primary qualities of matter; that it originally and properly accompanies certain sensations of the skin; that the proper conditions on which it occurs, are identical with the conditions which by experiment have been found necessary to the specific sensation of contact; and lastly, that the essential feature in the content of this perception is identical with the content of the special sense of touch. Is it not likely then, that what the advocates of Natural Realism really meant by the immediate perception of reality was, though indeed they were far from being aware of the fact, nothing more or less than tactual perception? If we did not know that there was such a thing as the special sense of sight, and if all we knew in connection with visual perception as sensation were the muscular sensations of the eye, it is quite probable that we would have a whole system of philosophy equal to that of Natural Realism and full of magic categories, all built up for the purpose of explaining the simple peculiarity of visual phenomena. And from the considerations which have just been enumerated it is reasonable to suppose that on just such principles has the central principle of the philosophy of Natural Realism been established for the purpose of express-

ing the peculiar, specific nature of the special phenomena of touch.

The results may be summarized briefly as follows:—
1. There is a general distinction between reflective and practical thought, the characteristic of the latter being that it consists in a great complexity of reactions to the belief in an external reality. 2. The psychological foundation of this practical sphere of thought can be found in the sense of touch. 3. The conception of contact has great influence on scientific hypotheses. 4. Owing to the current method and language of philosophy the influence and importance of touch in this sphere is not so apparent and has for the most part been overlooked, but there is one system in the history of philosophy which has endeavored to assert its claims, viz: Natural Realism. What the Scottish philosophers Reid, Stuart, and Hamilton, were striving to express in their doctrine of "immediate perception" of external reality was really tactual perception.

Valuable suggestions can be drawn both for psychology and for philosophy. From the psychological side we see the pressing need of a thorough investigation into the dermal and locomotor senses. These undoubtedly, form the greater part of the basis of practical life, and yet, uninvestigated and without language in which to assert their importance, they are almost wholly overlooked in the current interpretations. The language of touch is at best very vague and general. Moreover it is not indigenous, but formed analogically from the terms of other senses. What it needs is a language of its own, and the way to this is obviously by the methods of experimental psychology and a study of the evolution of the senses. The variety of senses included under what is generally termed the tactuomotor sense must be separated by experiment, the special nature and function of each determined, and the primitive history of their relations to life and the other senses thoroughly investigated.

From the side of philosophy we can in the first place agree with the Scottish philosophers that the philosophical systems current at their time, systems founded on the Lockian theory of ideas, were one-sided; that they were constructed almost wholly out of analogies taken from visual phenomena and consequently unintelligible and self-contradictory when employed in the expression of truths received by means of other senses. Natural Realism had a great truth to impress upon the world, but owing to the corruption of philosophical method and language it failed to get a hearing. It has been eclipsed in the history of philosophy by other apparently more attractive systems, not owing to the fact that they had the balance of

truth on their side but that they had the balance of language.

But the above results, I think, suggest a further and more important question as to the method of philosophy as a whole. If the fundamental category of the Lockian school of philosophy, the "idea," is really, as Reid and Stuart suggested, the "visual image," and if the central category of the Scottish school, "intuition" or "immediate perception" of reality is tactual perception, are we not led from this at least to hope for the possibility of finding a similar psychological statement for the chief categories underlying all systems of philosophy? Already it is generally admitted among philosophical critics that very large portions of every system of philosophy have a psychological foundation in the character of the age, nation and individual, and is it not equally probable that there is a like psychological basis underlying the very heart of each system, even unto the most fundamental and apparently ultimate categories? May it not be that all the magic categories of philosophy which profess to be ultimate expressions of the absolute, are only poetic attempts to express special feelings of sense which for want of attention and proper analysis are not recognized as such? The categories which have been handed down through the history of philosophy, taken as they must have been, from a comparatively chaotic mass of sensations, the elements of which were unseparated and uncriticized, must at best be very vague in their meaning and extremely inadequate to a scientific expression of the principles of life and thought. If a necessary element in the aim of philosophy is to keep itself in constant touch with the poetic æther, then its present system of categories may serve its purpose best, but if it means to bear a truly scientific attitude towards the world, it must forthwith surrender them up for psychological criticism, and that done, have itself restated in new and more scientific terms, rebuilt on a fresh system of more tangible categories all gotten from a thorough-going scientific analysis of instinct and sensation.

In conclusion I desire to express my gratitude to President Hall for kind criticism of my work and inspiriting suggestions during the preparation of this paper. I am also indebted to Dr. Scripture for reading over the paper and suggesting several corrections.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

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OBERSTEINER, *Die neueren Anschauungen über den Aufbau des Nervensystems*, Naturwissenchaftliche Rundschau, 1892, VIII, Nos. 1 and 2.

In less than half a dozen pages the author gives a very clear and judicial statement of the newer observations and theories which are the present guiding lines for research in the anatomy of the nervous system. Further comment is unnecessary except a word on a new term which appears in the article. For the most part anatomists take the view that the nerve cell and the nerve-fibre form a physiological unit and anatomically it is quite impossible to determine where one stops and the other begins. For the nerve cell and all its prolongations Waldeyer has suggested the term, Neuron. This fills so long felt a want and fills it so well, that there can be little question of its acceptance and hence the word of explanation.

LANGLEY AND SHERRINGTON, *On pilo-motor nerves*, Journal of Physiology, 1891, XII, 278.

The authors designate as pilo-motor those nerves which control the erection of the hairs or to use a single word, cause horripilation.

The experiments were made on a monkey, a young female Macacus rhoesus, and on cats. In all cases these nerves issue from the spinal cord by way of the ventral nerve roots and pass into the sympathetic ganglia; from there they are distributed to the skin.

The special arrangements are as follows: in the monkey the pilo-motor nerve-fibres for the head and face arise mainly from the third and fourth and less numerously from the second and fifth thoracic nerves. They pass cephalad in the cervical sympathetic and are connected with nerve cells in the superior cervical ganglion.

On stimulating the sympathetic nerve horripilation (in head and face) occurs chiefly on the homonymous side, but at the same time crosses the middle line to some extent.

On section of the sympathetic nerve the hairs lie abnormally flat in the effected region and remain so for many weeks.

In such a monkey anger and fear cause horripilation on the sound side only. The pilo-motor nerve-fibres issue in the roots of the twelfth thoracic, first, second, and third lumbar nerves, pass into the lumbo-sacral sympathetic chain and descend in it.

In the cat the pilo-motor nerve fibres are found in each nerve from the fourth thoracic to the third lumbar inclusive, sometimes also in the third thoracic. The fibres from the third or fourth to the seventh thoracic inclusive, run cephalad in the cervical sympathetic, join cells in the superior cervical ganglion, and innervate the skin on the head and

on the back of the neck. These fibres are either not present or not functional in all the cats examined.

The pilo-motor fibres from the seventh thoracic to the third lumbar nerves, supply a strip of skin about twelve cm. wide, extending down the middle of the back from the upper part of the thoracic region to a point some six cm. out on the tail.

The plan of innervation in this region is very interesting. It can be shown that stimulation of any spinal nerve root in this group causes horripilation along a strip of skin some ten cm. in length. Taking any two successive nerves the more caudal one innervates a strip of skin the beginning and end of which are about two cm. caudal of the strip innervated by the more cephalic nerve.

1. BECK, *Die Bestimmung der Localisation der Gehirn-und Rückenmarksfunctionen vermittelst der elektrischen Erscheinungen*, Centralbl. f. Physiol. 1890 IV 473.

2. FLEISCHL v. MARXOW, *Mittheilung, betreffend die Physiologie der Hirnrinde*, Ibid. 1890 IV 537.

3. BECK, *Die Ströme der Nervencentren*, Ibid. 1890 IV 572.

4. GOTCH UND HORSLEY, *Ueber den Gebrauch der Elektricität für die Localisirung der Erregungserscheinungen im Centralnervensystem*, Ibid. 1891 IV 649.

5. DANILEWSKY, *Zur Frage über die elektromotorischen Vorgänge im Gehirn als Ausdruck seines Thätigkeitszustandes*, Ibid. 1891 V 1.

The above mentioned papers are experimental, polemical and historical. They have grown out of the question, how far the activity of the central nervous system is accompanied by demonstrable electrical changes, and to what degree these changes can be used for the study of localization of function in it. Gotch and Horsley stimulated the cerebral cortex and noted the electrical changes in certain tracts of the spinal cord.

The others have for the most part applied a peripheral stimulus and noted the electromotive changes in the brain, mainly in the cortex. From the results of all, it would appear that the cortex is usually active to such an extent that there are continuous and irregular electrical changes, which can not be accounted for by distinct peripheral stimuli. Peripheral stimuli produce more or less marked changes in the resting current taken from the cortex and there seems to be some relation between the disturbance in the several sensory cortical centres and stimulation applied to their appropriate sense organs, but it is far from precise or satisfactory. On the power of anæsthetics (chloroform and ether) to prevent these electromotive changes, the authors are not in accord, Beck claiming that the spontaneous activity of the cortex continues under choloform, while v. Marxow claims that the cortex is paralyzed by anæsthetics.

All those who have employed the "negative variation" as an instrument wherewith to attack physiological problems are aware that it is a hard one to handle, and whether it can be used to add to knowledge of the functions of the cerebral cortex remains yet to be shown.

STEWART, *Notes on some applications in physiology of the "resistance" method of measuring temperature, with special reference to the question of heat production in mammalian nerves during excitation*, Journal of Physiology, 1891, XII 409.

The apparatus used registered changes in temperature by the swing of a galvanometer needle and in most experiments variations of 0.0005° C. could have been detected with certainty. Neither in frogs nor dogs and rabbits is there evidence of a variation of the above mentioned amount

in stimulated nerves. Hence such temperature changes as accompany the excitation of living nerves in these animals must be extremely small, if they occur at all. Likewise, nerves in the process of dying fail to show a change of temperature.

As bearing on the question of "thermogenic" as distinguished from "motor" nerves, it appears that the temperature of a muscle poisoned with curara does not rise on stimulation of the nerve, indicating that not only do the nervous impulses causing contraction of the muscle, but also those causing rise of temperature, (and they may or may not be one and the same) fail to effect the muscle, after curara.

The Johns Hopkins Hospital Reports, 1891, II, No. 6. *Report in Neurology, I.*

1. BERKLEY.—A case of Chorea insaniens, with a contribution to the germ theory of chorea.
2. SIMON.—Acute angio-neurotic oedema.
3. HOCH.—Haematomyelia.
4. THOMAS.—A case of cerebro-spinal syphilis, with an unusual lesion in the spinal cord.

In the papers above cited, the clinical and pathological points of view, as contrasted with the anatomical, are most emphasized. It will therefore be sufficient to mention here a few facts of very general interest connected with them. The study of chorea (1) is based on two cases—one a dog. In the first case towards the end of life, the chorea was associated with mental confusion. The post-mortem appearances to which the most value is attached were in the meninges and vessels, and are interpreted as the result of the action of a pathogenic germ or its products. To the numerous small extravasation of red blood corpuscles found in the nerve substances, but little significence is attached. Towards the end of the paper the changes occuring in the liver and kidney in diphtheria, are compared with those in the meninges, brain and kidneys in chorea, with a view to emphasizing the similarities and thus furnishing indirect evidence for the germ theory of chorea.

The disease designated as acute angio-neurotic oedema (2) is characterized by rather circumscribed swellings, appearing suddenly and often periodically, usually multiple and affecting the eyelids, lips, hands, feet, genitals, and buttocks by preference. There is often profuse vomiting. Three cases are carefully described. Vaso-mator influences alone appear insufficient to explain all the results, but as the disturbance is credited to the sympathetic system, these vaso-motor influences must be considered as one factor at least.

In the discussion of Haematomyelia (3) it is pointed out that hemorrhage into the spinal cord, not produced by trauma, is very rare. In the two cases described, while trauma is by no means excluded, yet the paralysis did not appear in one case until six days, and in the other until three weeks after the accident. The particular muscles affected were carefully studied, and from the probable location of the lesion the spinal centres for these several muscles is inferred, the inferences being controlled by what is already established in the localization of arm centres in the cord.

Dr. Thomas's case (4) yields the following anatomical summary. "Syphilitic orchitis. Syphilitic endarteritis (gumnatous) of cerebral arteries. Gumma on left third nerve involving crus. Gummata on left fourth, right sixth, ninth and twelfth nerves, and in brain. Gumma on anterior roots of three cervical nerves. Meningitis of cord. Poliomyelitis of lumbar enlargement. Hyaline degeneration in the walls of the small arteries." In the faithful account, both clinical and anatomical, which is given there, are a number of interesting points. No symptoms

were observed which corresponded with the marked changes found in the lumbar cord. The tumor on the right sixth nerve should have caused paralysis of the external rectus muscle of the right eye. The right eye had been tested shortly before death but no such paralysis was observed. On the peripheral side of the gumma this nerve did contain a number of well preserved nerve fibres, and this, too, in spite of the fact that the fibres could not be traced through the tumor. In the first place it is remarkable that the nerve should not have been destroyed, and in the second place that it should transmit despite the fact that the nerve fibre could not be traced through it.

WALDEYER, *Ueber einige neuere Forschungen im Gebiete der Anatomie des Centralnervensystems*, Deutsche med. Wochenschr. 1891 XVII 1213, 1244, 1267, 1287, 1331, 1352.

This review of recent work on the finer anatomy of the nervous system is from the hand of an acknowledged master. It is intended to show how far the improvements in the histological technique have, during the past few years, revolutionized the views on the architecture of the nervous system. The first paper starts with a historical review of the subject up to the year 1880, throwing into a scheme the ideas then current. Next follows a statement of Golgi's principle results. Undisputed is his observation that the nerve process (axis-cylinder process or prolongation) is branched, and that in certain cases it branches so much as to lose its identity within the gray matter about it. Disputed are his interpretation of the two sorts of cells as sensory and motor and his hypothesis that the branches from the nerve-process form a morphologically continuous net work throughout the nervous system and that the protoplasmic processes are purely nutritive in function. The points of difference in the views of the brothers Ramon y Cajal and Golgi are clearly stated. One very important point in the conception of the nerve cell is the value to be attached to the protoplasmic prolongations. There is much to be said in favor of the view that they possess functions not dissimilar in kind from those of the nerve process.

In order to form a picture of the arrangement of the elements in the spinal cord it is to be remembered that we have to deal within the cord with (1) commissure-cells (Commissurenzellen), (2) the column-cells (Strangzellen), (3) the nerve-root cells (Nervenwurzelzellen), (4) the cells of the dorsal cornua with the diffusely branching nerve-process. Outside of the cord lie the cells of the spinal ganglia. In general the relation of these elements appears to be the following: The fibres of the dorsal roots—for the most part taking origin from the ganglia of the dorsal root—enter the cord and there divide into an ascending and a descending ramus. From the rami arise at short intervals the so-called collateral branches which penetrate the gray substance and end in terminal brushes. The relation of these terminations to the cells is a close one but nevertheless not that of continuity. To follow the relations topographically, there are in the dorsal cornua the cells with the diffusely branching nerve process, the function of which is not evident. In the column of Clark, or Stilling's dorsal nucleus—as Waldeyer prefers to call it—the cells appear of the group designated as column-cells. Their nerve process passes to the lateral column and they may or may not divide into an ascending and a descending ramus. If undivided the fibre turns cephalad and, in any case, gives off collaterals along its course. In most parts of the gray matter cells of this class are to be found. The commissure-cells differ from the last only in the fact that the nerves coming from them cross the middle line by way of the ventral commissure before they turn longitudinally.

The root fibre cells form a final group. In most of them the nerve-

process gives off but a small number of branches near its origin and in some animals none at all have yet been discovered. The nerve processes for the most part pass into the ventral nerve roots but in some instances a group of cells mainly in the lateral portion of the ventral cornua send their nerve-processes to the dorsal roots. Thus some fibres in the dorsal roots arise from cells within the cord, a fact for which there has long been good pathological evidence. In the third paper the relations of the nerve elements in the cortex of the cerebellum and cerebrum is discussed. Some account of the relations in the cerebellum has already been given (this JOURNAL, Vol. III, No. 4, Feb. 1891). The account of matter in the cerebrum is taken from Ramon. The large pyramids whose pyramidal prolongation with its protoplasmic branches may extend almost to the sub-pial layer has a nerve-process which may contribute to any of the principal fibre-systems of the hemispheres and the branches of which also form modulated fibres as shown by Flechsig. Above and below the large pyramids lie triangular or small pyramidal cells, the lower layer of which have the peculiarity that their nerve process is directed towards the cortex. In the cortex of the rabbit S. Ramon has found bipolar cells and also triangular ones with several nerve prolongations both of which are entirely new elements in this region. In the fourth article it is principally the comparative anatomy of the nerve elements which is considered, especial weight being laid on the relations existing in the invertebrates. Without sufficient grounds, as it seems to us, Waldeyer concludes in favor of direct anastomoses between cells in both vertebrates and invertebrates. In the crustacea which he examined Retzius considers almost all the ganglion cells as monopolar. The nerve process however has lateral branches, but no nerve fiber takes its origin from these branches. In the crabs the sensory and motor fibres appear to arise in the same manner and we have then both sorts of cells centrally located and in physiological connection by the lateral branches of the nerve-process.

These general views find support in the arrangements within both the olfactory and optic apparatus. Here there is evidence for two sets of fibres originating from separated groups of cells and running in opposite directions. In the case of the olfactory it would appear that some cells of the sensory nasal epithelium are cells of origin for nerve fibres passing from them to the bulb. One question of prime importance in connection with the supporting tissues of the nervous system is whether these are derived from the epiblast alone or from this layer and the mesoblast. This is one of the matters discussed in the fifth paper and although it must be still left open, the evidence appears to be strong that these tissues have a double origin. In the final paper the various points discussed are brought into a general view and illustrated by schemata. Some of the principle conclusions are the following. The axis-cylinder of all nerve fibres is the direct outgrowth of a nerve cell and in no case does it arise out of a net work of fibres. All nerve fibres end free in a terminal-brush and in no case is there formed an anastomosing net-work. The entire nervous system can be considered as built of a series of units. Each unit is the cell and its outgrowths. This unit Waldeyer designates as a neuron. As a rule the order of arrangement is such that physiological connection is established by the terminal brush of one neuron expanding in the neighborhood of the cell-body of the next. The arrangement in the glomeruli of the olfactory bulb suggests that in certain cases the terminal brushes of two nervous-processes may be directly approximated. In considering the value of the nerve cells, Waldeyer discusses the hypothesis of Nansen that the nerve cells are simply nutritive and do not form part of the pathway for the nervous impulses, coming to the conclusion that the evidence for Nansen's view is at present insufficient.

Two very general matters may be mentioned in conclusion. First, up to recently it has been generally held that the method of silver impregnation depended solely on a deposit of metal in lymph-spaces, to this Waldeyer adds a possible staining of certain elements which if it means anything means that the reaction takes place within the substance stained and not around it. Second, Kölliker in discussing this subject has laid great stress on the question how far the fibres brought out represent those which are modulated and how far those which are non-modulated. On this point Waldeyer has nothing to say.

OBERSTEINER, *Anleitung beim Studium der Baues der nervösen Centralorgane im gesunden und kranken Zustand*, 2. Aufl., Leipzig 1892.

The first edition of this admirable work was received with general rejoicings and was, at the time, reviewed in these columns. (AM. JOUR. OF PSYCHOLOGY, Vol. II, No. 2, Feb. 1889.) Since then (1890) it has undergone translation at the hands of Dr. Alex. Hill, of Downing College, Cambridge, England. The translation is good and the English edition differs from the original German in containing certain addenda, (always bracketed in the text), in which, for the most part, the translator presents some morphological views of his own. We do not propose to attempt here more than to point out some features of the second German edition as compared with the first. The fundamental character and arrangement are unchanged; as the author tells us in the preface, the text has been carefully worked over. The result is about one hundred pages more of reading matter and several new cuts.

A first-class book of this kind is in some sense a work of art and as such must have its sketchy portions. At the same time it is sure to be judged by what is best in it which, in this case, is the anatomical matter —in the stricter sense of the term. Where the evidence for views is physiological or developmental, the author's critical sense is less helpful to the reader. We have said that the book has grown and that in parts it is sketchy; it is to be devoutly hoped that it may remain so and stop growing. Even in this second edition there are introduced new things, presumably for the sake of completeness, which weaken its character as a critical essay. It seems the fate of many strong books to thus undergo in later editions a form of fatty degeneration where bulk is gained and tone is lost, and the perspective of the subject is damaged. Turning now to details, several matters call for notice.

The section on methods is fuller and more accurate than before. Take it all in all this chapter forms the best manual on the histological methods for the nervous system that we have. In discussing the method of degeneration it does not appear why Schwalbe's hypothetical nerve fibre with two nutritive centres should be introduced. It represents a purely formal difficulty. The development of the central nervous system is just touched upon and histogensis is hardly mentioned. In the chapter in morphology Fig. 20 is not without fault. The lateral plexus appears to be cut off from the rest of the velum and the stria cornea is represented on one side only.

In considering the fissuration of the hemispheres the author holds closely to Ecker. Eberstaller's contributions to the subject are recognized in the text and we should be glad to see his boundaries for the occipital lobe accepted in the figures. These figures (24-27) can certainly be improved. The central fissure should cut the mantel-edge and the relations of the interparietal sulcus and parieto-occipital fissure in Fig. 24 are quite misleading. The parieto-occipital fissure normally cuts the mantel-edge much in front of the point at which it is indicated. And furthermore the figures do not agree among themselves in representing this relation.

At birth the fissuration in the normal brain is usually almost complete, only the tertiary sulci being in part undeveloped and the statement that "the principal fissures are present," leaves the completeness quite unemphasized; for the erroneous idea that the fissuration of the hemispheres at birth was still far from finished, Ecker is mainly responsible.

In alluding to localization in the cortex Obersteiner falls back on Exner's view that they are foci without sharp limits. However true this may be for the lower mammalia, the recent work on man and the higher monkeys points to a sharper limitation in these higher forms, so far, at least, as the motor centres are concerned. The law given for the relation of the thickness of the cortex to the size of the gyri, according to which the larger gyri have the thicker cortex, certainly does not include the insula, for there the gyri, however considered, are of moderate size though the cortex is the thickest. Further, deep sulci are found in the occipital region where the cortex is thin, and gyri with broad tops, though bounded by shallow sulci, are found on the orbital surface where the cortex is equally thin. The large gyri in this connection must therefore have both broad tops and deep sulci bounding them in order to present the thicker cortex.

In discussing brain weight, if the figures are taken from Bischoff, as they appear to be, the average weight for the female brain should be 1220 grms. instead of 1230 grms. as printed. It should further be made clear that these mean weights are obtained from brains still enclosed in the leptomeninges.

Direct evidence given by Topinard, Bischoff, Boyd and others indicates that some brain growth takes place up to thirty-years in females, while in males it may continue nearly ten years longer. This fact is hardly suggested by the statement that towards the twentieth year the maximum brain weight is attained. In presenting Wagner's figures for the superficial extent of the cortex it should be stated that the measurements were made on brains shrunken by alcohol and that while the specimens of Wagner's series are comparable among themselves, his figures do not without correction form a basis for determining the extent of the cortex in the fresh brain.

It is agreeable to find the question of the difference in the weight of the two hemispheres of the brain properly neglected. There is no doubt that some brains have one hemisphere larger than the other. There is equally little doubt that the differences usually found depend on the difficulty of dividing the brain fairly and that these difficulties, arising from the distortion of the specimen and the constant error of the operator—who cannot possibly divide even a favorable object into two equal portions,—give rise to those inequalities which have so often been treated as important, but which in reality lie well within the limit of the errors of observation. The results of Obersteiner's own careful observations on the specific gravity of different portions of the brain form the last topic discussed in this chapter on the morphology. In taking up the histological elements of the nervous system the author states the fibrilla and hyaloplasma theories of the structure of the axis-cylinder without attempting to decide between them. While considering the axis-cylinder as continuous he decides, on the strength of Jacobi's results, against the discontinuity of the sheath of Schwann, as advocated by Boveri. In man he claims a considerable degeneration of fibres on the central side of lesions occuring in a peripheral nerve,—adding that this degeneration is less marked in animals. Further a normal degeneration of peripheral nerves in man, implying a degeneration of the cells connected with them, is asserted. Such an idea surely needs more evidence for its support than can at present be furnished. In discussing the nerve-cell he adheres to the older views. The absence of chromatin

from the nucleus of the nerve cell is noted and the cell outgrowths discussed with special reference to the axis-cylinder prolongation as demonstrated by Golgi's method. The author seems to us unduly skeptical on this particular point, though his arguments against the simple nutritious functions of the protoplasmic prolongations have more force. Our principle indictments against this chapter is that he still speaks of nerve-cells and fibres as separate elements, thus failing to utilize the valuable conception of the cell and its fibres as forming both a morphological and physiological unit.

Beginning with the spinal cord we come to the most valuable portions of the book. Here Cajal's results are freely used. The view that the columns of Goll are pathways for the muscle sense is supported by the observation that these columns are poorly developed in the limbless forms. The segmental nature of the spinal cord is passed over on the ground that it is but faintly indicated in the higher mammals—not a very sufficient reason.

In discussing the spinal nerves their double origin—from both sides of the cord—is described and this idea is carried over to the cranial nerves where even the patheticus and abducens are forced into the schema. One cannot help feeling in the light of v. Guddins's results, that the weight of evidence is against such a view. The new figures (134-136) in the section on the medulla and interbrain form welcome illustrations of the latter region. If an argument were needed to show how much the histologist had yet to do on the nerve centres, no better one could be offered than the fact that the olfactory bulb and tract are here illustrated and considered from the examination of them in the dog. The contribution of His to the make up of the olfactory bulb is not mentioned and the double nature of the optic nerve is passed over. Farther on, the anatomical myth about the fibres of the Callosum joining identical points of the cortex appears. This is pure hypothesis and should not be presented as anything else.

Finally, the pictures illustrating the cortex (p. 445) are all out of drawing. The size of the cells and the relative thickness of the several layers are both calculated to give wrong impressions, which are only in part to be corrected by the figure on p. 451 illustrating the distinction of fibres in the cortex.

References to the more important literature have been introduced at the end of each section and in many cases the abbreviations used in connection with the figures have been arranged in alphabetical order in the explanation, thus facilitating reference. The foregoing remarks are intended simply as a running comment to the thanks due the author from those who have occasion to use his lucid and instructive book.

TURNER, *The convolutions of the brain; a study in comparative anatomy*, Jour. of Anat. and Physiol., 1890-1, XXV, 105, also *Verhandlungen des X. internationalen medicinischen Congresses, Berlin 1890*, II. Berlin 1891.

This paper is valuable for the simple and novel form in which it presents the comparative anatomy of the gyri. Lacking, as we do, a really adequate theory of the formation of the gyri from the physiological side, it is necessary to come back to the comparative anatomy for the significance of these foldings; from this latter standpoint our author reviews the field.

He makes departure from the very general fact that a cerebral hemisphere is separable into two natural divisions—a ventral portion, or Rhinencephalon, and a dorsal portion, or Pallium. These main divisions are separated by the rhinal or ects-rhinal fissure.

So far as the rhinencephalon is concerned Turner follows Broca in making it the basis for a further grouping. Instead of Broca's two

groups of osmatic and anosmatic animals, Turner makes a threefold division into macrosmatic, represented by the Ungulata, Carnivora, etc., microsmatic represented by the Pinnipedia, whalebone-whales, apes and man, and finally the anosmatic, represented by the dolphins, toothed-whales, etc.

In ascending the mammalian series the pallium developes the more rapidly and thus more and more overgrows the rhinencephalon. As a result the rhinal fissure passes from a lateral position in the lower forms, to a ventral one, in the higher. The main subdivisions of the rhinencephalon are the bulb, peduncle and lobus hippocampi. Two roots of the peduncle are described; these bound the quadrilateral space. Of course with the variations in the size of both pallium and rhinencephalon the topographical relations of the latter may be various. The lobus hippocampi is in general larger in the microsmatic animals than in the anosmatic, but it is still present in the gross form in the latter. Supposing that in this last case it contains normal nerve elements, we are in the position of being forced to explain a special-sense centre which has no peripheral connections. There seem two ways out of such a position; either to find that the hippocampal lobes are histologically abnormal in the anosmatic forms or that this region has some other function in addition to a centre for smell. It is not improbable that both these notions would be involved in any complete explanation. In all animals the pallium is larger than the rhinencephalon, the difference being greatest in the higher forms. Species in the same order may have in some cases a convoluted pallium, in others a smooth one. The insectivora are apparently the group in which the surface of the hemispheres most perfectly preserves its smoothness throughout life in all the genera. In the monotremata, Ornithorhyncus has a smooth brain while that of Echidua is convoluted After considering the orders in which the pallium is slightly convoluted our author notes the order of appearance of the fundamental fissures in these forms and shows that, while the sylvian fossa is to be associated with the rhinencephalon, the sylvian fissure belongs to the pallium. Further among the lissencephala—or smooth-brained forms—the sylvian fissure is by no means necessarily the first to appear. In many forms there is a tendency to the formation of a sagittal fissure and marginal gyrus before the sylvian can be recognized. Taking the fundamental fissures it is evident that there is no fixed order for their appearance but that the order differs according to the groups of animals examined.

For purposes of general description Turner groups the fissures into sagittal, arcuate and radial, terms which hardly require further explanation.

Taking up first those orders in which the convolutions are most complex, it appears that the representatives of smallest size in which the brain is large as compared to the body, may be lissencephalous, as for example in the case of little marmoseb monkey among the Primates. In the carnivora, pinnipedia, cetacea and ungulata certain accurate fissures arch over the sylvian—itself to be classed with the radial fissures —and in the most typical cases form three concentric gyri, which, enumerated from the sylvian fissure outwards, are the sylvian, suprasylvian and marginal gyri. Where it is deep, the sylvian fissure always hides the Insula.

In the carnivorous brain the crucial fissure is a characteristic feature extending from the mesal surface outwards and bounded at its lateral end by the sigmoid gyrus. The mesal surface in this group has a well marked splenial fissure—both longitudinal and accurate in its course, having important relations to the crucial fissure just named.

The homology of the fissure of Rolando with the various fissures in

the carnivorous brain with which it has been compared are merely mentioned and the author passes on to propose the question whether there can not be an occipital lobe without a parieto-occipital fissure and decides that there can be if the caudal prolongation of the lateral horn, the post. cornu, is taken as the criterion. Again he argues for the recognition of both frontal and parietal lobes even where the fissure of Rolando is absent. It is plain from what has been said that the convolutions can have very little value in determining phylogenetic relationship and that their significance is not fundamental. The remaining pages are devoted to the various theories of the formation of the convolutions. This is the least satisfactory portion of the paper. It should be added that there are more than forty cuts interpolated in the text, many of them representing the brains of unusual or rare animals.

II.—ASSOCIATION, REACTION.

PROF. J. MCK. CATTELL,
Columbia College.

HÖFFDING, *Ueber Wiederkennen, Association und psychische Activität.*, Viertelj. f. wissensch. Philos. 1889 XIII 420; 1890 XIV 27, 167, 293.

LEHMANN, *Kritische und experimentelle Studien über das Wiedererkennen*, Phil. Stud. 1891 VII 169.

WUNDT, *Bemerkungen zur Associationslehre*, Phil. Stud. 1891 VII 329.

JASTROW, *A statistical study of memory and association*, Educational Review, 1891 II 442.

RIBOT, *Enquête sur les idées générales*, Rev. Philos. 1891 XXXII 376.

MUENSTERBERG, *Zur Individualpsychologie*, Centralbl. f. Nervheilkunde, 1891 XIV 196.

DUMAS, *L'Association des idées dans les passions*, Rev. Philos. 1891 XXXI 483.

The papers by Prof. Höffding, Dr. Lehmann and Prof. Wundt are intimately related to each other, as well as to preceding articles and text-books. Höffding criticises Lehmann's previous paper "Ueber Wiedererkennen" (Phil. Stud. 1888, V 96-156), in which the latter maintained that it is not necessary to assume similarity as a principle of association. Höffding argues for the integrity of association by similarity, laying special stress on the recognition of previous experiences. In such cases the recognition is the psychological correlate of the greater mobility of the corresponding molecules of the brain. A change which has once taken place occurs the more readily the second time. Lehmann argues that every experience is complex, and that the recognition, even of a comparatively simple sensation, is due to contiguity rather than similarity. Wundt in view of these papers and of Scripture's recent experimental study explains and elaborates the doctrine of association contained in the third edition of his psychology. He holds that simultaneous association should be ranked co-ordinate with successive association, and that the latter depends, as the name itself indicates, on the continuous interweaving of all the ideas under the control of conciousness. We may look on the disagreement of our leading psychologists in these questions without great anxiety, for after all the matter is largely one of nomenclature.

Turning to the experimental results of Lehmann's paper, we find them to be of interest. In his first section he gives the results of 428 trials on

the recognition of smells. In 45% of the trials an immediate association was called up, in $\frac{1}{4}$ of which the observer was apparently mistaken as to the nature of the smell. In 28% of the trials a name was immediately suggested, which was wrong $\frac{1}{3}$ of the time. In 7% of the trials the smell was recognized, but called up no association nor name, and this class Lehmann considers the most interesting theoretically. The second section is on the recognition of sounds. Lehmann finds, as Starke and Merkel had previously found, that the intensity of the second of two sounds is overestimated. Lehmann thinks that this is because the actual sensation must appear stronger than a memory-image. On this assumption the longer the interval between the two sounds, and the less exact the memory-image of the first sound, the greater should be the overestimation of the second sound. The experiments given in support of this theory do not seem to confirm it very well, but Lehmann thinks the variations are due to a periodicity in the fading of the memory-image. It is not, however, a matter of course that because a sensation is remembered less exactly, it should be represented as weaker. The complex effects of memory, contrast and fatigue cannot be satisfactorily explained on this theory. The writer of this notice finds that the second of two weights is, indeed, overestimated, but the second of two lights is still more regularly underestimated. It may be suggested that it would be more convenient and accurate if writers who know mathematics (as Lehmann does) would give probable errors and not merely the number of mistakes made in 60 or 1380 trials.

Prof. Jastrow publishes the results obtained with his classes in the University of Wisconsin and on students in the Milwaukee High School. He gave ten words separately to the students, and obtained the first associated words. After 48 hours he required them to write down as many of the original words as they could remember. Then he gave them the original words, and required them to write down as many of their former associations as they could remember. He obtained the interesting result that, while about $\frac{1}{2}$ of the words were forgotten, only $\frac{1}{5}$ of the associations were forgotten. Some of the words were remembered much better than others, whereas the associations on the several words were remembered about equally well. In the classification of the associations the great frequency of certain associations (pen-ink, cat-dog, etc.,) is apparent. There were only 241 different associations in 700 cases. About $\frac{1}{2}$ the associations come under the heading "natural kind, or one object suggesting another of the same class." After this division "whole to part" was the largest. The results obtained with the university students and with the school students were much alike. The women remembered better than the men, and their associations were the less diversified. This illustrates an important distinction, which obtains throughout the animal kingdom—the greater variability of the male.

Prof. Ribot gave words to 103 persons, and recorded the suggested ideas. "Nothing" was suggested 53% of the whole number of times! The observers were classified according to the nature of their mental imagery. The visual type in which a more or less distinct image of the object is called up was the most frequent. The types in which printed words were seen, or auditory images prevailed, were rare. Prof. Ribot formerly called attention to the importance of movement to imagination, but in the present paper nothing is said concerning those whose thoughts are chiefly accompanied by the impulse to spoken words or other movements, to which class Prof. Stricker and the writer of this notice belong.

Dr. Münsterberg has made tests concerning association and various mental traits in school children and others. In the present paper he describes the methods he has used, but does not as yet give his results. The tests suggested by Münsterberg need not be described here until

their appropriateness has been demonstrated by the publication of these. But one cannot fail to honour the heroic perserverence which is borne witness to by experiments of this sort.

The paper by M. Dumas does not contain experimental results.

MARTIUS, *Ueber die Reactionszeit und Perceptionsdauer der Klänge*, Phil. Stud. 1890 VI 394.

MARTIUS, *Ueber den Einfluss der Intensität der Reize auf die Reactionszeit*, Phil. Stud. 1891 VII 469.

Dr. Martius here continues the publication of careful experiments on reaction-time carried out in his private laboratory at Bonn. In his first paper he gives experiments showing that the reaction-time becomes shorter as the pitch of a tone is taken higher. A monochord was used to produce the tones, and the times were measured with the Hipp chronoscope. C^{i}, c^{i}, c^{ii}, c^{iii} and c^{iv} were used, and the times compared with those obtained from the noise made by a hammer and anvil In a general way the times are the same (in the neighborhood of 110σ) for c^{iv} as for the noise, and about 40σ larger for C^{i}. There are considerable differences with the three observers, which are probably due to the limited number of experiments, 12 to 19 of each sort, with an average variation of about 10σ. Martius concludes from a comparison of the reaction-times that 1 to 4 vibrations are sufficient to call up a sensation.

Prof. Stumpf in reviewing this paper (Zeitsch. f. Psych. II, 230-232) suggested that the difference in time of the reaction might be due to the greater intensity of the higher tone. Martius consequently made experiments in which the intensity was varied, and obtained as result that there is no difference in the length of the reaction-time for sounds of different intensities. This is contrary to the results obtained for several classes of stimuli by Wundt, Exner, v. Kries u. Auerbach, v. Vintschgau u. Honigschmied, v. Wittisch, Berger and the writer of this notice. Martius thinks that this discordance is explained by the greater attention given in his experiments, but it more likely due to the small range of intensity. The intensity of the sounds was not measured, but in no case can a monochord give a very loud sound.

REPSOLD, *Neuer Vorschlag zur Vermeidung des persönlichen Zeit-Fehlers bei Durchgangsbeobachtungen*, Astronomische Nachrichtungen 1889 Dec. 9, No. 2940.

BECKER, *Ueber einige Versuche von Durchgangsbeobachtungen nach dem neuen Repsold'schen Verfahren*, Astronomische Nachrichten 1891 May 19, No. 3036.

LANDERER, *Sur l'équation personelle*, Comptes rend. 1889 CVIII 21.

GONNESIAT, *Sur l'équation personelle dans les observations de passages*, Comptes rend. 1891 CXII 207.

STROOBANT, *Recherches expérimentales sur l'équation personelle dans les observations de passage*, Comptes rend. 1891 CXIII 457.

ANDRÉ ET GONNESIAT, *Etude expérimentale de l'équation décimale dans les observations de passage, faite à l'Observatoire de Lyon*, Comptes rend. 1892 CXIV 157.

CHRISTIE, *Change of personal equation with stellar magnitudes in transits*, Monthly Notices of the Royal Astron. Soc. 1891 455.

BACKHUYZEN, *Bestimmungen der persönlichen Gleichung bei Passagebeobachtungen*, Viertelj. d. Astron. Gesellsch. 1889 249.

A personal equation machine, The Sidereal Messenger 1891 139.

The photochronograph and its application to the star transits, Georgetown College Observatory 1891 36.

Prof. Wundt and Prof. Exner have called attention to the psychological interest of the personal equation long known to astronomers, and

Dr. Sanford has given in this JOURNAL a thorough historical and critical review of the subject up to 1888. Since then astronomers have continued the study of a subject so essential to their science. The accuracy with which time can be measured, and with which position and motion can be determined, is dependent on the personal equation, and reduction of the error by .01 sec. would be an important advance in astronomy. The papers by Dr. Repsold and by Prof. Becker suggest a new method for eliminating or lessening the personal equation. Repsold had previously proposed that the transit instrument might be moved at the same rate as the star, and the observer might at his leisure adjust the wire so as to bisect the star. If the position of the instrument were known, the time of transit could be measured very exactly. But the mechanical difficulties of moving and adjusting the instrument proved insurmountable, and Repsold now proposes that the wire only should be moved by the observer. This is done by means of a screw, and the position of the wire is registered automatically on the chronograph. A number of such registrations can be taken at short intervals during a single transit. Repsold tested his method artificially, and found the variable error of observation to be comparatively small (44σ to 27σ), and the constant error to be nearly eliminated. Becker tested the method in actual transit observations with less satisfactory results. The personal differences of four observers were obtained by the chronographic method and by Repsold's method, and the results were reduced in the usual manner. The probable error was found to be one-fourth to one-fifth larger by Repsold's method. It seems likely, however, that improvements in the apparatus and practice on the part of the observers would make this new method the most accurate hitherto used.

Of the reports in the *Comptes rendus* that by M. Stroobant has the most psychological interest. He calls attention to the fact, familiar to psychologists, but not systematically applied in astronomy, that an observer can judge when his registration is worse than usual. The writer of this notice finds in recent experiments that the error in adjusting a movement is about half as great as the error in perceiving it. The error in adjustment is perceived as an error and may be eliminated, the total error being thus reduced by about one-eighth. Stroobant finds that the "eye and ear" method is not much less accurate than the chronographic method. The first limb of a planet (artificial) was registered too soon (115σ to 238σ), and the second limb too late (16σ to 52σ). Registrations become later as a sitting is continued. Stroobant finds that he has a considerable decimal equation, and this error is more elaborately studied by MM. André and Gonnessiat. They find it to be about .05 sec. The decimal equation (first noticed by Prof. B. Pierce), is due to giving preference to certain decimals in estimating parts of seconds or millimetres. It would seem to be an error difficult to eliminate. Observers differ greatly, and an observer's knowledge of his own error would probably lead to its alteration. M. Landerer discusses the part which diplopia (doubling of the image) may play in transit observations. This defect can of course be corrected by glasses, but it may be increased by fatigue during the observation. The British astronomer royal gives the alteration in the personal equation with a change in the brightness of the artificial star. The star was darkened by gauze netting, but the alteration in intensity was not measured. The registration tended to become later, as the star was made less bright.

Prof. Backhuyzen explains his method for studying artificial transits, which is described in detail in Vol. VII of the annals of the Leiden Observatory. In this method the artificial star is stationary, and the apparent motion is obtained by a revolving prism. The Eastman personal equation apparatus is described anew in the *Sidereal Messenger*. This is the only artificial transit apparatus which the writer has examined

and he does not know how it compares with others. But it is one of the most recent and considered one of the best. The technical advances in psychology are borne witness to by the fact that the psychologist would not like to use a recording apparatus whose error is over 1σ, whereas the variable error of this instrument is about 20σ, and the constant error is not entirely eliminated by the ingenious method of reversing the motion of the carriage.

The paper last on this list is the most important for astronomy, but does not especially concern psychology. As long ago as 1849, Faye suggested the possibility of recording transits by photography, and this has now been actually accomplished. It is not necessary to describe here the methods and apparatus used in the Georgetown College Observatory under the general direction of the Rev. Father Hagen. Stars of the fourth magnitude have been successfully photographed in transit, what Prof. Young calls the "annoying human element" being largely eliminated. The photographic method will probably be applied with great advantage to many physical measurements.

The interests of psychology are not especially served by any of these papers. Astronomers naturally wish to do away with the personal equation rather than to study it. The most important advances have been in this direction. The Repsold method transfers the error to a certain extent from the observer to the instrument, and the photographic method does away with the observer altogether during the actual transit. The work of astronomers becomes less important for psychology as their devices become more mechanical, and as psychology itself learns to state and to solve its own problems. On the other hand recent advances in psychology are of increasing importance to astronomy and the other physical sciences. Physical measurements in the last resort must always depend on the accuracy of the eye and hand. Errors of observation are now studied in psychology with an exactness which has never been approached in any physical science. There are but few physicists and mathematicans who understand the position of psychology in this matter. The physicist cannot know the true value of the quantity he is seeking to determine; he deals with residuals not with errors. The psychologist on the other hand determines actual errors, and can study their nature, size and dispersion in a manner entirely beyond the reach of physics. The whole theory of the method of least squares is concerned with variable errors, and is helpless in the presence of constant or systematic errors. Constant errors are, however, far more important and dangerous than variable errors, and these can be measured and eliminated by the psychologist. Astronomers have, indeed, attempted this with their artificial transit instruments, but they have been playing the part of the psychologist, in most cases without adequate methods or knowledge.

ARDIGÒ, *Alcune osservazioni relative alla legge psicologica del riconoscimento*, Rivista di filosofia scientifica 1891 X 577.

The author relates an experience in the reproduction of a dream which seems to support his theory of re-cognition. He presents several considerations on cognate points from which he deduces two consequences, the one in regard to the association of ideas and the other relation to the theory of reasoning. The former denies that the process of association is the revival of terms one after the other that exist separately in the organic predisposition of the cerebrum, but asserts that it is a re-enlivement little by little in various parts of an ample system which acting in its integrity from one point to the other does so in successive moments and with variations of intensity in different parts. Thus it is

deduced that the principle of association is the same for simultaneous and successive associations and for those of similarity; moreover we see that there are two species of simultaneous and successive associations, the direct and the indirect. The direct association of coexistence and sequence takes place because the single system preformed as a physiological synergy is aroused in its integrity, reacting successively in its parts. The association by similarity takes place because the special rhythm of such an entire system stimulates analogous rhythms of other systems physiologically preformed, in the same way that a piano-string in vibration produces resonating vibrations in other strings of analogous rhythm. The indirect association of coexistence and sequence takes place because the rhythm of activity, when there is consciousness of one term of an associative series, arouses the analogous rhythm of a term of another associative series in such a way that the whole physiological system takes part in it. Reasoning is nothing more than a product of the law of re-cognition.

E. W. SCRIPTURE.

SCRIPTURE, *Ueber den associativen Verlauf der Vorstellungen*, Inaug. Diss. Leipzig 1891; also Phil. Stud. 1891–2 VII 50.

The first step to a scientific treatment of the subject must be a careful collection of material instead of the fictitious examples generally in use. The course of ideas in consciousness can for the sake of scientific study be divided into four processes: preparation, influence, addition and posterior effect. The process of preparation is the change which an apperceived idea undergoes before it influences the course of consciousness. In one form of association the whole of the apperceived idea acts and remains in the result; e. g. the word Kothe calls up the phrase "in Kothe," (p. 17). In another form the whole of it evidently acts but the resulting idea does not contain it: e. g. touch-impression from a piece of paper—word "paper," (p. 17). Often only part of the apperceived idea is of effect, that is, it is diminished by the concentration of the attention on certain parts which are active in producing the result whereas the other parts are apparently lost; this is the process of the diminution of an idea. Example, Rahm—Raum; the association is caused by the three letters while the other disappears (p. 20). The second fundamental process is the influence of ideas on the course of consciousness. It is of two kinds, direct and indirect; the former is the case where an idea produces a change without the intervention of another idea; example, "ach!"—"ach, weh!" taste of lemon juice—word, "lemon juice;" sound of a tuning-fork—visual image of a tuning-fork (p. 26). The other form is the indirect influence. Sir Wm. Hamilton thinking of Ben Lomond associated to it the apparently unconnected Prussian system of education; he had, however, once met a German on that mountain and the association can be explained by supposing the unconscious links of association thus: Ben Lomond—the German—Germany—Prussia—the Prussian system. To test the point by experiment, a series of cards was prepared on half of which were German words, *A*, *B*, *C*, *D*, and some unknown Japanese letters, u, v, w, x; the other half contained Japanese words in Roman characters, *M*, *N*, *O*, *P*, with the same Japanese letters, w, u, x, v. The series having been shown in this way, one of the German words was then exposed without the Japanese letter and the observer was to notice on what he next thought. The Japanese letters were generally forgotten and the Japanese word in Roman characters was often associated without the observer knowing why. The probability of the correct Japanese word being associated to the German word was about one to five; actually this occurred in the ratio of nearly three to two, or, if some cases where other influences were at work be omitted, in the ratio of two to one. Experiments with other combinations of ideas, e. g.

words, colors, names, etc., seldom give such results, the direct influence being generally the more powerful. The forgotten or semi-forgotten Japanese letters were to be found in various degrees of consciousness, and several pages are devoted to an investigation of them. The third process, addition, can be illustrated by the following examples: The—Thee; sound of two pieces of wood rubbed together—visual image of the small pepper-boxes (which grind) at table in a Swiss hotel (p. 44). The addition of elements to an idea often takes place while the idea itself undergoes a diminution as above described; this may go so far that none of the original idea is left, every substitution is thus an addition with diminution. A large collection of examples is given illustrating the various forms of the process. One of the most interesting points is the addition of the coefficient of recognition (first noticed by Höffding); the simplest form is seen in the example: touch-impression from a piece of silk—recognition of an indefinite touch-impression, (p. 57). The development of the quality of recognition into localization in space and time is illustrated by numerous examples. The Herbartian revival of ideas and the English reproduction of ideas are impossible terms, ideas being neither revived nor reproduced; the facts are limited to the existance of an idea at a given moment which exhibits certain properties that we attribute to previous occurences in consciousness. These properties are called after-effects. One peculiar case is experimentally investigated, namely, the effect of an unperceived element. A series of picture cards is shown with such short exposures that only the picture is seen while a letter in the indirect field of vision entirely escapes notice. Then the letters are shown singly and the observer is asked to say what picture belongs to each of them. The results show that the unapperceived portions of an idea are sufficient to call up the idea. The bearing of these experiments and those on indirect influence in explaining cases of apparently disconnected successions of ideas is evident. AUTHOR.

III.—HYPNOTISM AND SUGGESTION.

PROF. J. JASTROW,
University of Wisconsin.

BÉRILLON, *Les faux témoignages suggérés chez les enfants*, Rev. de l'Hypnotisme 1892 VI 203.

Dr. Bérillon recounts some observations on children both in the waking and hypnotic conditions. He asks a child to pay especial attention to his words and says: "you will forget your name;" the child is really unable to speak its name, although evidently struggling to do so. Another boy ten years of age is asked to tell what he did the day before, he mentions that a Mr. J. was present at dinner, when he is interrupted with the statement that his memory is confused and that he doesn't know whether Mr. J. was present at lunch or at dinner. His mother asks him to remember but the recollection is gone. Another boy 12 years of age is told that when in the street yesterday he saw two men fighting, the one struck the other and fled. The man was large and so on. Upon questioning the lad recalls the whole scene and will not believe it was suggested to him. Another child similarly is made to accuse a respectable neighbor of theft, or accuse his schoolmate of assaulting him and so on. The suggestions are often extended by the immagination of the subject. Dr. Bérillon concludes that with children from six to fifteen years of age it is easy by simple affirmation

either in the waking condition or in hypnotism, to promote illusions of perception, partial amnesia, distortions of memory and hallucinations. The realization of such suggestions in children, is the rule and the failure the exception. The readiness with which these phenomena may be utilized for inducing false testimony is obvious and should be taken into account in all legal cases in which the testimony of children is admitted. As evidence of the sincerity of the children Dr. Bérillon offers his own impressions, and the fact that in many cases the suggestions were realized at the first meeting and when the children were in ignorance of the expected result. They were selected from all classes of the population, and Dr. Bérillon is of the opinion that the intelligent children are more rather than less susceptible to its influences. The great suggestibility of the children seems clearly related to the great prevalence of good hypnotic subjects in France, and it may be questioned whether a similar condition of suggestibility would be found amongst the children of our own country.

GUÉRIN, *Considérations juridiques à propos des faux témoignages suggérés*, Revue de l'Hypnotisme 1892 VI 212.

The French code punishes false testimony with the same penalty that attaches to the accused if convicted by such testimony, and although the testimony of children is under special regulations, the possibility of injustice by suggested testimony is not diminished thereby. Just as the inebriate is responsible for the effects of his passion when he first indulges it so the subject of suggestion is responsible for allowing himself to be the subject of suggestion. "He is as culpable for accepting criminal suggestions as he would be for following bad advice; the situation is the same." The danger for the accused is extreme, and it is the business of the students of hypnotism to furnish means whereby the suggested may be distinguished from the true experiene and whereby the author of the suggestion may be discovered. Equally important is the necessity of limiting these practices to physicians and allied scientists.

VOISIN, *Délit de vol commis sous l'influence de la suggestion hypnotique*, Revue de l'Hypnotisme 1892 VI 219.

A woman aged twenty, subject to hystero-epilepsy, catalepsy and somnambulism was arrested for stealing many objects from the Magasins du Louvre. It appeared that for three months she had been stealing with extreme adroitness at the suggestion of some accomplices. At the same time her suggestibility in the waking condition was so great that her companions could make her do and believe almost anything. On recommendation she was sent to the Salpêtrière instead of to prison and was there restored mainly by suggestion.

GOIX, *Anorexie hystérique traitée avec succès par la suggestion hypnotique*, Revue de l'Hypnotisme 1892 VI 245.

Anorexia is the persistent refusal of food and may result fatally; the sole cause is that the patient does not want to eat. Marie Ch..... aged 23 appears September 10 before Dr. Goix and has not eaten for four days; during this time her energy and industry are extraordinary. Hysterical symptoms are clear. She is hypnotised but refused to promise while hypnotised, that she will eat. The next day while hypnotized she drank a cup of chocolate which she is told is water, (water she takes at all times), but still refuses the suggestion of eating. Still later the suggestion is given her that she will repeatedly say "I will eat, I will eat." By repeated suggestion, setting the time of eating, threatening her with severe pain, the opposition is at last broken down and a normal

appetite ensues. The case is offered as showing the importance of the precise formula of suggestion, the need of special adaptability to each case and the possibility of administering food during the hypnotic condition.

ARTIGALAS *et* RÉMOND, *Note sur un cas d'hemorrhargies auriculaires, oculaires et palmaires, provoquées par suggestion*, Revue de l'Hypnotisme 1892 VI 250.

The patient, Mme. F., aged 22 years, after an operation (uterine tumor) quite unexpectedly manifested hysterical symptoms. The most remarkable of these was the shedding of tears of blood. Hypnotic suggestion at first failed to stop these, while it was quite sufficient to say "you will bleed in a minute" to promote the phenomena. Again hypnotized it was suggested that she should bleed in the palm of her left hand. A bloody perspiration followed in a few minutes. Then it was suggested that the bleeding would stop at the palm and also at the eyes, and in this way she was speedily cured. The case is naturally brought into relation with the cases of stigmatisation and it is easy to see that in combination with a religious order this symptom might have been given a mystical significance.

MACDONALD, *Traumatic Hypnotism*, Science 1892 XIX 23.

The account tells of a physician who was thrown out of her cart and suffered a contusion on the right parietal protuberance over the third descending convolution. The last thing the patient remembered was calling to a man to get out of the way. The report of others shows that after the accident she said she was not hurt, washed her face and hands, gave directions and answered questions. For a moment she awakes but relapses into this condition again, delirium also ensues. Mr. MacDonald regards this as a case of traumatic hypnotism.

WRIGHT, *Traumatic Hypnotism*, Science 1892 XIX 66.

Describes the case of a boy thrown off a horse against a barn door, who thereupon arose, finished his farm duties, went to the house, took a light supper and answered questions; he seemed entirely normal except for a vacant start and an occasional senseless laugh. He went to bed and on awakening next morning was found to have no memory of anything after the accident. The author regards this as a case of spontaneous hypnotism, differing from the preceding one in that the patient does not pay any attention to the accident but goes on automatically with his routine work.

BALDWIN, *Suggestion in infancy*, Science 1891 XVII 113.

Do ideo-motor or suggestive re-actions, have any part of normal mental life or is the hypnotic sleep to which this may be affiliated essentially artificial? As a contribution to this inquiry Prof. Baldwin utilized his observations of his child during her first year. If ideo-motor suggestions are normal then early child life should present the most striking analogies to the hypnotic state in this respect. Three kinds of suggestions are distinguished: 1. physiological, 2. sensori-motor, 3. ideo-motor; this being the order in which they appear in child-life. 1. The meaning of physiological suggestion is sufficiently shown by one of the observations. For the first month or six weeks the life of the child is mainly physiological, the vacancy of consciousness as regards anything not immediately given as pleasure or pain precludes the possibility of ideal suggestion as such; no ideas in the sense of distinct memory-images are present. Yet suggestions of sleep began to tell on the child before the end of the second month.

She was put to sleep by being laid face-down and patted. This soon became not only suggestive of sleep but also an indispensable suggestion. 2. Among the sensori-motor suggestions we find various sleep suggestions, food and clothing-suggestions and suggestions of personality. For the next month there was an increasing power of the sleep suggestion just mentioned. In the mean time two nursery rhymes were added. In the third month a difference was noticed between the effect of the suggestions coming from the nurse and those from another person. In the fourth month the father succeeded with difficulty in substituting his suggestions for those of the nurse although they were imitated with the greatest care. The sleep suggestion thus depended on the personality of the nurse—the peculiar voice, touch, etc. The power of the father was gradually developed, succeeding at night better than in the day-time; darkness was thus an additional suggestion. A single flash of bright light causing a closure of the eyes was often a most powerful suggestion. At this time other persons had great difficulty in producing sleep, whereas the father succeeded in a short time. At the end of a year the child would voluntarily throw herself into position at a word and would go to sleep, if patted, in from four to ten minutes. At 16 months even when the nurse is unable to do anything with her the mere sight of the father makes her quiet and in five minutes put her to sleep. This illustrates the passage of a purely physiological suggestion into a sensory one. The sight of the rubber on the end of the food-bottle—not the bottle alone—was suggestive of movements as early as the fourth month; the touch of the bottle with the hands was not suggestive till later. At the fifth month the sight of mittens, hood and cloak caused signs of joy. (The referee has noticed a case at the same age where the sight of the mother with a bonnet on at once produces quiet when the child is restless, the restlessness returning if the mother departs; whereas the same does not occur if no bonnet is worn.) The ideo-motor suggestions are of two kinds: deliberative and imitative. By deliberative suggestion is meant a state of mind in which co-ordinate stimuli meet, affront, oppose, further, one another. A most instructive case is reported showing the conflict between the impulse to scratch and the idea of the punishment, the latter gradually overcoming the former. Imitative suggestion is of two kinds: simple and persistent. Illustrations of these will at once occur to the reader. In conclusion the facts of suggestion as stated from the nervous side are as follows: Physiological suggestion is the tendency of a reflex to get itself associated with and influenced by other sensory or ideal processes; sensori-motor suggestion is the tendency of all nervous re-actions to become secondary-automatic and reflex; deliberative suggestion is the tendency of different competing sensory processes to merge in a single motor re-action, illustrating the principles of nervous summation and arrest; persistent imitative suggestion is the tendency of a sensory process to maintain itself by such an adaptation of its re-actions as to transform them into new stimulations. From the side of consciousness, suggestion in general is the tendency of a sensory or ideal state to be followed by a motor state.

E. W. SCRIPTURE.

Studies in hypnotism at Brown, The Brown Magazine 1891 III 1.

In the course of some experiments on hypnotism at Brown University two cases occur that are of interest. The first shows the resistance of the subject to post-hypnotic suggestion and his way of avoiding a seemingly ridiculous action. The subject was told that on waking he should say *ee* instead of *ä*, as "feether" instead of "father." When awakened he was asked: "Is one of your parents living?" "Yes, sir." "Your mother?" "Yes, my mother and—and—" he apparently tried

to say "father," smiled and added "and—both of them." "But you were about to say your mother and—?" "My mother and—and—her husband." The second case is that rare occurrence, auto-hypnotism. One of the students can sit down, lay out a certain course of action, hypnotize himself, performed the predetermined operations, return to his seat and wake up. While in this state, no outside personality has any influence over him. He has used this power several times to induce sleep at night, waking as usual the next morning. On one occasion feeling rather exhausted he dropped into a chair and said he would hypnotize himself, in order to feel well upon awakening. Accordingly he did so, and after about forty-five seconds awoke declaring that his head felt much better, though his body was still tired. It is to be regretted that the observations were conducted for their popular interest more than for their scientific value; it is to be hoped that this case of auto-hypnotism will be more carefully observed and described.

E. W. SCRIPTURE.

IV.—SIGHT.

GREEFF, *Untersuchungen über binokulares Sehen mit Anwendung des Heringschen Fallversuchs*, Zeitschrift fur Psychologie and Physiologie der Sinnesorgane, 1892 III 21.

This is a more careful test of Hering's experiment with the falling balls in order to determine the accuracy of our perception of the third dimension and the conditions upon which it depends. The apparatus used by Hering and the conditions of the experiment were somewhat modified by Dr. Greeff, but only with a view to greater mathematical accuracy in the results, and to a greater variation of the circumstances under which the judgment of observer was to be formed. The distance between the eyes and the point of fixation was made definite and measured. A screen was employed so as to make the angle at which the falling ball could be seen the same for all the experiments. Also a perforated screen was placed above the line of vision with the holes in it at regular distances which were measured. The balls were dropped through these perforations and the judgments of the observer recorded with the known and definite distance of the falling ball from the point of fixation whether before or behind it. The design in shutting off from view a part of the distance of the falling ball and including only that came within the limits of a given angle was to exclude the influence of occular movements upon the judgments of localization in relation to the point of fixation. The observer looked through a conical shaped roll of paper with the inner surface darkened, and the apex or smaller end farther from the eye in order to prevent the entrance of disturbing rays of light into the eyes. This conical tube was about 30 cm. long, and the wider end about 20 cm. wide. The point of fixation in a box of 60 cm. length and 20 cm. width was situated 95 cm. from the eyes. This distance, however, seemed to vary with the conditions necessary to produce the parallel position of the eyes by means of a prism before one of the eyes. This expedient was resorted to in order to remove the force of the supposition that a convergent position of the eyes had something to do with the judgments of localization. In all his experiments Dr. Greeff found that at all distances the judgment of distance was as correct when the eyes were in a parallel position as when convergent. The observer's confidence and certainty were as great in one case as in the other. The 2 to 3 per cent. of failures he attributes to the fluctuations of attention and the coincidence of winking with the fall of the ball. The most noticeable feature is the marked difference between monocular and binocular vision in regard to the correctness of the judgment of distance. The first set of experiments represents four

different conditions with a common point of fixation 1.12 cm. from the observer. The first 50 trials were with monocular and the second 50 with binocular vision and without the use of prisms. The third set was with the aid of a prism to produce the parallel position of the eyes, and the fourth set of 60 trials was with prisms that produced a slightly divergent position of the visual axes. The judgments represent the relative localization of the falling ball compared with the point of fixation. Following are the results:

	No. of Balls.	Right Judgments.	False Judgments.
Monocular vision,	50	26	24
Binocular vision, free,	50	49	1
Binocular vision, with parallel axes; Prism 7,	50	50	—
Binocular vision, with divergent axis; Prism 8,	20	18	2
Prism 10,	20	20	—
Prism 12,	20	17	3
Prism 14,	20	8	12

The prisms were placed before the right eye and were multiplied in order to produce the utmost degree of divergence possible. The results are three-fold. *First*, the increased accuracy of binocular over monocular vision. *Second*, the equal accuracy of all three positions of the eyes in the judgment of relative distance; namely, the convergent, parallel, and divergent positions. *Third*, the coincidence of error with that degree of divergence which overcomes the tendency to fusion. When the same general principles were observed and the point of fixation was made 60 cm., of 200 falls in monocular vision 51 per cent. was false, and 200 in binocular vision only 1.5 per cent., 98.5 per cent. being correct. This set of conditions was assumed as a starting point for increasing the distance of the point of fixation in order to see what the limit of correct localization would be when the balls fell the same distance before or behind the point of fixation. When the last was made 1 m. the percentage of failures was still 1.5 per cent; at 1½ m. it was only 1 per cent. At 2 m. the failures increased to 3 per cent. and at 3 m. they were 6 per cent. This result suggested two changes in the experiment. First, the enlargement of the angle through which the ball fell; and, second, the increase of the distance from the point of fixation for the falling ball. It was found that with the enlargement of the angle of vision and thus of the visual field under the same conditions as above the failures decreased down to 2.5 per cent. when the distance between the edges of the screen was 20 cm. This was for binocular vision. For monocular vision under these last conditions the errors were 4.3 per cent. But with the fixation point 3 m. distant and starting with 1 cm. distance from that point for the falling ball this distance was increased up to 10 cm. when the percentage of errors had fallen to 2 per cent. from 6 per cent. This relation was more particularly determined by a set of experiments with two different persons and the same general result obtained. The general conclusion reached by Dr. Greeff was that the localization was proportioned in its definiteness and accuracy to the ratio between distance of the fixation point from the eyes and the distance of the falling ball from that point. The matter was then more carefully tested by the study of cases in which one of the eyes was either naturally or artificially affected by influences that diminished the distinctness of the images: naturally by maculae corneae, cataracta incipiens, amblyopia congenita, etc.; artificially by powdered lenses.

The results in localization were much as in the first case. The number of errors was remarkably small, being for 100 trials 2 per cent. when the fixation point was 1 m., 3 per cent. when it was 2 m., 5 per cent. when it was 3 m., 18 per cent. when it was 4 m., and 39 per cent. when it was 5 m., and the left eye representing a distinctness of $\frac{6}{12}$ to $\frac{6}{6}$ for the right eye. With the ratio of $\frac{6}{6}$ for the right and $\frac{1}{36}$ for the left eye, and the distance of the fixation point 1 m. the errors were 3 per cent., and 20 per cent. when the distance of the fixation point was 2 m. The distinctness of vision, therefore, according to Dr. Greeff, has very little to do, within moderate limits, with the perception of relative distances.

Columbia College. J. H. HYSLOP.

HERING, *Prüfung der sogenannten Farbendreiecke mit Hülfe des Farbensinnes excentrischer Netzhautstellen*, Archiv. f. d. ges. Physiologie 1890 XLVIII 417.

At the time of the publication of Hess's study of the peripheral color sense Hering based upon it a critique of the Young-Helmholz theory (both papers reviewed in this JOURNAL, III, 203, 204). The present paper is a continuation of that critique with particular reference to the color-triangle of König and incidentally to those of Maxwell and Fick. Hering finds one and all of them and indeed all possible color-triangles made upon the Young-Helmholz theory out of harmony with the facts established by Hess. The discussion is technical and for it the reader is referred to the original. In the latter part of the paper he also shows the irreconcilable opposition between the observed brightness of colors seen with the periphery of the retina and the three-color theory, citing in part the results of a study of that subject, also by Hess, presently to be published. The reply of Fick to his former paper, reviewed in the JOURNAL, III, 574, reached him to late for special rejoinder, but he considers its points answered in the present paper, and counts upon Fick as a convert when he shall have investigated the matter by Hess's method.

E. C. SANFORD.

KLOBUKOW, *Vorlesungsversuch zur Demonstration der Wirkung von Complementärfarben und Farbengemischen beim Zusammenbringen von gelösten Farbstoffen*, Ann. d. Physik u. Chemie 1891 XLIII 438.

In explaining the effects of mixing colors to a large audience it is very desirable to have direct mixtures and not those produced by the color-discs. It is proposed to have colored solutions of the desired shades so prepared that they are not soluble in and cannot take color from one another but have a great difference of specific gravity. Two solutions are shaken together in such quantities that the desired color is produced. The mixture is than allowed to stand a short time, at the end of which the two component colors are found separated one above the other. For example, to show the effects of a mixture of red and green a solution of aldehydegreen in amylalcohol and one of cobalt-salts in water are used. If the proper strengths are employed the mixture is a dirty white. The addition of common salt to the cobalt solution hastens the separation of the two. For mixtures of blue and yellow a solution of phenanthrenchinon or some other derivative of chinon that is insoluble in water but soluble in amylalcohol and an ammoniacal solution of copper in water are to be used. The mixture is a bright green and serves to show that mixtures of pigment colors are different from those of spectral colors. Likewise a solution of chinon in amylalcohol and a combination of the solutions of cobalt and copper in water (as near as possible to the violet of the spectrum) will give a dirty white when mixed in the proper proportions, whereas the corresponding spectral colors are not complementary.

E. W. SCRIPTURE.

OTTOLENGHI, *Nuove osservazioni sul campo visivo in psicopatici*, Archivio di Psichiatria, Scienze Penali ed Antropologia criminale, 1891 XII 112.

Ottolenghi's complete memoir of part of which a résumé is here given will appear as Vol. IX of the *Biblioteca Antropologica Gluridica.* The cases examined, numbering about 60, include criminals, neurasthenics, hypnotic subjects, epileptics and prostitutes. The numbers are too small to base general theories upon, but some very interesting facts were brought out by investigations. In the case of criminals the irregularity of the field of vision,—more or less accentuated limitation, and vertical hemiopia,—was found to be the chief characteristic. In neurasthenics the field of vision was limited, but the perimetric line was regular and the limitation concentric for colors.

From his observations on five hypnotic subjects, Ottolengni concludes: 1, that in case of especially hysterical hypnotic subjects the field of vision can be normally extended; 2, in a state of monoideism, of psychic exaltation, the limits of the field of vision are much extended, but do not go over the physiological limits; 3, in the hypnotic state the field of vision does not vary notably. The variations depend upon the state of psychic excitation in which the subject finds himself. When the subject is in a tranquil somnambulistic state, the field of vision varies least in extension but it can become irregular if the subject does not readily perceive or is fatigued. The field of vision is modified by the suggestion which is exerting its influence on the subject. The greater the sensitiveness and excitability of the subject, the more regular is the periphery of the field of vision.

In the cases of four occasional criminals (women) only one case of limitation of the visual field was met with and no irregularity of the periphery. In the cases of four male occasional criminals, only two had the field of vision slightly limited. Amongst ten typical female criminals, however, all but two showed more or less limitation of the field of vision. Of eleven typical prostitutes, eight had limitation of the field of vision, eight irregular, and four broken perimetric lines. In the cases of four hysterico-epileptics the field of vision was limited, but the perimetric line was always regular; in one case lateral hemiopia was met with. Of 13 young criminals (boys), the field of vision was limited in twelve, and in six the perimetric line was broken. In five cases the limitation assumed the form of partial vertical hemiopia. Amongst eight epileptic boys the field of vision was considerably limited for both the eyes in five cases, in four cases the periphery was irregular, and in three cases partial vertical hemiopia was found. Ottolenghi considers that these results cast doubt on the opinion of Schule that in epileptic children the field of vision is not injured. He considers that these new observations confirm the fact ascertained before that the extension and the regularity of the field of vision follow very faithfully the variation of the psychic state of the individual.

A. F. CHAMBERLAIN.

BOSTWICK, *Estimates of distance*, Science 1892 XIX 118.

The reproduction of small distances is not influenced by immediately preceding reproductions. The mean variation in estimates depends on previous training.

E. W. SCRIPTURE.

A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

BY EDMUND C. SANFORD, PH. D.

(*Third Paper.*)

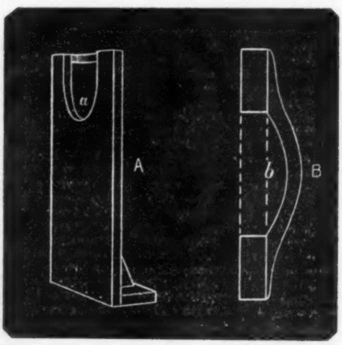
V.—VISION.

THE MECHANISM OF THE EYE, AND VISION IN GENERAL.

Apparatus. Many of the experiments of this section can be performed with very simple apparatus, made on the spot. The following materials will be needed: Pins, cards, corks, a candle, a couple of postage-stamps, a watch glass, pieces of colored glass, black and white card-board (not shiny), colored papers, a light wooden rod. Four inches square is a convenient size for the glass, of which two pieces should be cobalt blue, one red. Any colored papers will serve; those made for artificial flowers are easy to get in large variety of tints. A fine series of papers in Helmholtzian colors is sold by R. Jung, Heidelberg. In addition to these supplies there is need of a double convex lens of short focus, two inches or more in diameter; an ordinary burning or reading glass would do, though those mounted on an adjustable stand, costing $2.50 and upward from the physical instrument dealers, are more convenient; also a concave spectacle lens.

For Ex. 99 a pink-eyed rabbit and a little modeling clay are necessary.

An instrument for facilating Ex. 103 (a Phakoscope) can be had from Jung for 25 marks; a more elaborate instrument of the same name is quoted by the Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, England, for £ 8-8.

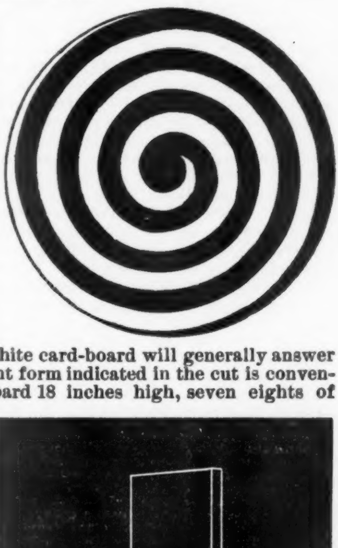


For Ex. 109 and other experiments a firm head rest of some sort is required. For most purposes one like that shown in the cut will answer well enough and can easily be made. Fig. A shows a board about 20 in. high and 12 in. wide with a U-shaped opening cut in the top to receive the face, the chin resting at *a*. Fig. B, the top view, shows the cross piece against which the forehead rests at *b*. The whole when in use is clamped to the edge of the table. When a complete immobility of the head is desired it is best secured by providing a thin board cut out so that it can be put into the mouth and taken between the jaws. If the parts upon which the teeth rest are covered with sealing wax and are bitten upon while the wax is still soft, not only is a firm support for the head secured, but the head can be returned again exactly to its former position after an interval, if desired. Such a mouth board could

easily be added to the support shown in the cut. For pictures of such mouth boards cf. Hermann, Handbuch der Physiol. III, pt. 2, pp. 440, 473 and 478, also Helmholtz, Optique physiologique, p. 665 (p. 517 of the first edition), Aubert, Physiologische Optik, p. 647.

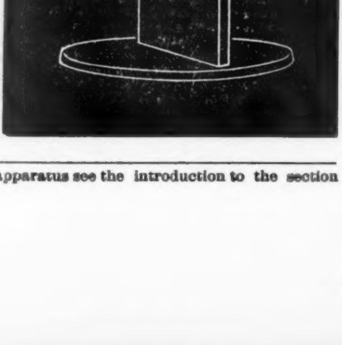
For Ex. 110 make a saturated solution of chrome-alum in water, filter and put into a flat-sided clear glass bottle. Dilute, if necessary, till the yellow spot can be observed as described in the experiment.

Ex. 115 requires a pair of electrodes and a battery. The electrodes can be made by soldering connecting wires to plates of brass or zinc, two and a half inches wide by three long, and covering them with cloth. Some kind of a key for opening and closing the circuit and a commutator for changing the direction of the current are helpful, though not essential. Any battery giving a sufficiently strong current will do; one of four cells of the "gonda" pattern has proved sufficient for demonstrative purposes, and a very much weaker one will serve for showing the flash from electrical stimulation.



Ex. 119, involves a rotation apparatus of some kind and a disk traced with a spiral as in the cut. Any rotation apparatus will do, but if the laboratory is supplied with batteries one of the small electric motors now to be had at a very low price is easily adaptable for use and is extremely convenient.[1] A Porter motor, retailing at $3.00, has been used with success in this laboratory. It is well to have the disk large, a foot in diameter, and the line of the spiral thick, three eighths of an inch across, and a good black.

In a number of experiments black or white screens are to be used. A simple piece of black or white card-board will generally answer but sometimes the more permanent form indicated in the cut is convenient. It consists of an upright board 18 inches high, seven eighths of an inch thick and 12 inches wide, firmly fixed on a wooden base. In the cut the base is made too large. One side of the upright is covered with black card-board (or painted a dull black), the other with white card-board.



Of models helpful in understanding the mechanism and functions of the eye there are a number. Anatomical models are quoted, among others, by Jung (12 marks); by Kny & Co., 17 Park Place, New York, ($5.00 to $28.00); by Queen & Co., 924 Chestnut St., Philadelphia (Auzoux models, $19.00 and $20.00 without duty). Of physiological models, the best for accommodation and the like is Kühne's op-

[1] For fuller information on rotation apparatus see the introduction to the section on color-vision, to follow.

tical eye made by Jung, at 65 marks, by the Cambridge Scientific Instrument Co., at £7. The action of the muscles and the behavior of the eye in motion is illustrated by the Ophthalmotrope, described with cut by Helmholtz, Optique physiologique p. 678, (p. 527 in the German edition). This instrument is to be had of Jung, at 25 marks. of the Cambridge Scientific Instrument Co., for £10; and of other dealers also. Another instrument for the same purpose, called the Blemmatotrope is described by Hermann in Pflüger's Archiv, VIII, 1873, p. 305. The motions of the eye and their effect on the retinal image, such especially as those mentioned in Ex. 123, are finely shown by the Phenophthalmotrope of Donders, described in v. Graefe's Archiv für Ophthalmologie, Bd. XVI, 1870, and sold by Jung at 30 marks. An improved form of the instrument is to be had of D. B. Kagenaar, Rijks-Universiteit, Utrecht, at 40 guilders. Suggestions for simple illustrative apparatus will be found with the description of the experiments.

Standards and rods with clamps and universal joints, thought not distinctively for visual experiments, are by far the most important of the general conveniences of a laboratory. They enter into the setting up of very many experiments and a liberal share of even a small appropriation may well be invested in them. Ordinary clamps can be bought in all sizes at the hardware stores at prices from ten cents upward. The standards and couplers to be had from the chemical and physical instrument dealers are made for another purpose and are not very satisfactory in the psychological laboratory. Those made for physiologists and photographers are better. Wilhelm Petzoldt, Bairische Str. 13, Leipsig, makes a considerable variety, of which the following have been found useful in the physiological and psychological laboratories of Clark University. Standards: simple tripods with interchangeable rods of 9 and 13 mm. diameter, 6.50 marks, and large tripods with leveling screws in two of the feet and carrying two of the above mentioned rods, at the same time, 16 marks. Taple-clamps, which screw on to the edge of the table and are bored to receive the rods, thus taking the place of tripods: two kinds, one bored for the 9 mm. rods, but having only a vertical hole, 2.75 marks; the other bored for 13 mm. rods having both horizontal and vertical holes, 3.50 marks. Couplers to fit both sizes of rods: those for the 13 mm. rod (of iron) and connecting the rods only at right angles, 2 marks, those for the 9 mm. rods (of brass) and connecting the rods either at right angles or parallel 2.75 marks. Petzoldt also makes small clamps of various sizes, like those furnished with the chemical sets, mounted upon the 9 mm. rods, at 3 marks. The advantage of these rods and couplers is that they fit nicely and can be set up so as not to wobble. By using several rods and couplers a universal motion can be secured, but not so conveniently, as by the ball-joint clamps and swivel couplers made for photographers' use by Otis C. White, of Worcester, Mass. These allow extreme freedom of movement, and when fastened do not slip nor wobble. The ball joints are made to clamp on the edge of the table or to screw upon the end of rods. The first can be had in great variety of sizes, a convenient one fitting half inch rods costing $1.25. The swivel couplers allow the coupling of the rods in any position relative to each other, those of size to connect half inch and quarter inch rods costing 50 cents. Rods of various diameter and length may also be had with the ball-joints and swivel clamps. In purchasing for a laboratory from several makers it would be well to fix upon standard sizes for rods and fittings so that all may be interchangeable; and also to fix upon a standard size and number of threads to the inch for all screws cut upon the rods so that any clamps, pulleys or other small pieces of apparatus, made to screw upon one, will fit all.

On Vision in general cf. Helmholtz, Handbuch der physiologischen Optik. (The second German edition has reached page 400 : the latest complete edition is the French translation, Optique Physiologique, Paris. 1867). Aubert, Grundzüge der physiologischen Optik, Leipzig, 1876, (a portion of Graefe and Saemisch's Handbuch der ges. Augenheilkunde). Le Conte, Sight, New York, 1881. Beaunis, Nouveaux Éléments de Physiologie Humaine, Paris. 1888, (Beaunis, like Helmholtz, gives bibliographies). Wundt, Physiologische Psychologie, II, 82-209. Hermann's Handbuch der Physiologie, Bd. III, Th. 1, Leipzig, 1879.

The references following the experiments below are made chiefly to Helmholtz, the pages of the new German edition, the French edition, and, in parenthesis following the latter, of the first German edition being given, but the experiments of this section are more or less fully discussed in almost all of the works just mentioned and in many others besides.

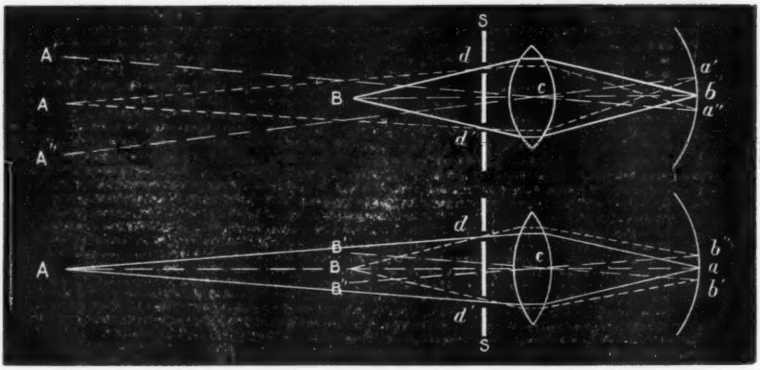
99. **The retinal image.** The mechanisms of the eye accomplish two things: the projection of a well defined image on the retina; and the ready shifting of the eye so as to bring successive portions of the image into the best position for vision. The retinal image is readily seen in the unpigmented eye of a pink-eyed rabbit. Chloroform the rabbit, remove the eyes and mount them in clay for readier handling. Make a thick ring of clay with an internal diameter a little greater than that of the cornea of the rabbit's eye, place the eye cornea downward in the ring and lay a similar ring upon it to keep it in place. It can now be handled easily and turned in any direction. Turn it toward the window and from behind observe the inverted image on the retina. Bring the hand into range and move it to and fro; observe that the image of distant objects is more distinct than that of the hand. If convex and concave lenses are at hand (spectacle lenses will answer) bring them before the eye and observe that the effect upon the retinal image is similar to that seen subjectively when they are held before the observer's own eye. Reverse the eye, holding it retina side toward the window, and observe the radiating and circular fibres of the iris. The eye must be fresh, for if long removed it loses its transparency.

100. **Accommodation.** The sharpness of the retinal image depends on the adjustment of the crystaline lens, which must be such as to focus the light from the object under regard upon the retina. The lens must be thicker and rounder for near objects, thinner and flatter for more distant ones. These adaptations of the eye are known as *Accommodation*. The changes in the clearness of the retinal image are easy to observe subjectively. Hold up a pin or other small object six or eight inches away from the eyes. Close one eye and look at the pin with the other. The outline of the pin is sharp, but the outlines of things on the other side of the room behind it are blurred. Look at these and the outline of the pin becomes blurred. Notice the feeling of greater strain when looking at the nearer object. The experiment is somewhat more striking when the nearer object is a piece of veiling or wire gauze and the farther a printed page.

On this and the next two experiments cf. Helmholtz, Physiologische Optik, 2nd Ed. pp. 112-118, French ed. pp. 119 (90)-126 (96).

101. **Accommodation. Scheiner's experiment.** *a.* Pierce a card with two fine holes separated by a less distance than the diameter of the pupil, say a sixteenth of an inch. Set up two pins in corks distant respectively eight and twenty inches in the line of sight; close one eye and holding the card close before the other with the holes in the same horizontal line look at the nearer pin; the farther pin will appear double; look again at the nearer pin and while looking cover one of the holes with another card; one of the images of the farther pin will disappear, the left when the left hole is covered, and the right when the right is covered. Look at the further pin or beyond it and repeat the covering, covering the left hole now destroys the right image of the nearer pin, and covering the right destroys the left. Why this should be so will be clear from the diagrams below. The upper diagram illustrates

the course of the rays of light when the eye is accommodated for the nearer pin; the lower diagram when it is accommodated for the farther pin. *A* and *B* represent the pins; *S* and *S* the pierced screen; *d* and *d'* the holes in the screen; *c* and *c* the lens; *a'ba''* and *b''ab'* the retina; *A'*, *A''*, *B'*, and *B''*, the positions of the double images; the solid lines the course of the rays from the pin accommodated for; the dotted lines the course of the rays from the other pin; the lines of dashes the *lines of direction*, *i. e.*, those giving the direction in which the images appear to the observer. In the upper diagram the rays from *B* are focused to a single retinal image at *b*, while those from *A*, being less divergent at first, are brought to a focus nearer the lens, cross over and meet the retina at *a'* and *a''*, and since each hole in the screen suffices to produce a retinal



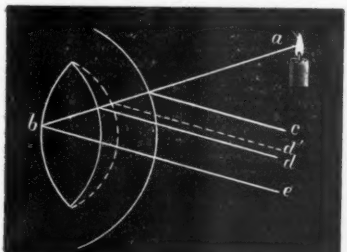
image, cause the pin to appear double, and its two images are referred outward as usual with retinal images along the lines of direction, (which cross a little forward of the back surface of the lens, in the *crossing point of the lines of direction*), the right retinal image corresponding with the left of the double images and *vice versa*. If now the right hole in the screen be closed the left retinal image and the right double image disappear. The case of accommodation for the farther pin will be clear from the lower diagram, if attention is given to the dotted and dashed lines. It will also be easy to explain why moving the card when looking through a single pin hole causes apparent movements of the pin not accommodated for, and why in one case the movement seems to be with the card and in the other case against it. *b*. Stick the pins into the corks so that they shall extend horizontally, and examine them with the card so held as to bring the holes above one another. *c*. Arrange the holes thus: . • . and observe that the triple image of the nearer pin (when the farther is fixated) has the reverse figure • . • Scheiner's experiment can easily be illustrated with a double convex lens and a pierced screen of suitable size.

102. The Range of accommodation. *a*. Find by trial the nearest point at which a pin seen, as in Ex. 101, can be seen single. This is the *near point* of accommodation. For the short-sighted a *far point* may also be found, beyond which double images reappear. *b*. Find how far apart in the line of sight two pins may be and yet both be seen single at one and the same time. Try with the nearer at 20 cm., at 50 cm., at 2 m. That portion of the line of sight, for points in which the same degree of

accommodation is sufficient, is called the *line of accommodation*. The length of the line increases rapidly as the distance of the nearer object from the eye increases.

Cf. Helmholtz, *op. cit.* G. 114, 119, Fr. 122 (93), 123 (97).

103. The Mechanism of accommodation. *a.* The change in the lens in accommodation is chiefly a bulging forward of its anterior surface. This may be observed as follows. Let the subject choose a far and a near point of fixation in exactly the same line of vision, close one eye and fix the other upon the far point. Let the observer place himself so that he sees the eye of the subject in profile with about half the pupil showing. Let the subject change his fixation at request, from the far to the near point, being careful to avoid any sidewise motion of the eye. The observer will then notice that more of the pupil shows and that the farther side of the iris seems narrower. This change is due to the bulging forward of the front of the lens. If the change were due to accidental turning of the eye toward the observer the farther edge of the iris should appear wider instead of narrower. *b.* Purkinje-Sanson images. The changes in the curvature of the lens may also be observed by means of the images reflected from its front or back surfaces and from the front of the cornea. Operate in a darkened room or at night. Let the subject choose far and near fixtation points as before. Let the observer bring a candle near the eye of the subject at a level with it and a little to one side and place his own eye in a position symmetrical to the candle on the other side of the subject's line of sight. Careful examination will show three reflected images of the flame; one on the side of the pupil next the light, easily recognizable, bright and erect, reflected from the surface of the cornea; a second nearer the centre of the pupil and apparently the farthest back of the three, erect like the first, but very indistinct, (more like a light cloud than an image), reflected from the anterior surface of the lens; and a third, a mere point of light, near the side of the pupil farthest from the flame, inverted and reflected from the posterior surface of the lens. When the observer has found these three images the subject should fixate alternately the near and far points chosen. As he fixates the near point the middle image will grow smaller, advance and draw toward the corneal image; when he fixates the far point the image will enlarge, recede and move away from the corneal image. The llowing diagram after Aubert illustrates the movement of the middle image; the full lines indicate the positions of the cornea and lens and the course of the rays of light when the eye is accommodated for the far point; the dotted lines indicate the anterior surface of the lens and the direction of the ray reflected from its surface when the eye is accommodated for the near point. Three images similar to those in question can be observed on a watch glass and a double convex lens held in the relation of the cornea and crystaline.



Cf. Helmholtz, *op. cit.* G. 131-141, especially 131-134, Fr. 142 (104)—154 (112), especially 142 (104)—146 (107). Aubert, Physiologische Optik, 4 44.

104. Chromatic aberration. Of the various defects of the eye as an optical instrument only one will be mentioned here, namely, chromatic aberration, and that because it has been supposed to offer a possible means of inferring the relative distance of objects from the eye. The

different colored rays of light are not equally refracted by the lens, the violet most, the red least, and the other colors in order between. The point at which parallel violet rays are brought to a focus is therefore nearer the lens than the point for red; and in order that the same degree of accommodation may serve to show a red lighted object and a violet lighted object at the same time and both with full distinctness, the red must be somewhat farther away. *a*. The aberration can easily be observed by looking at a small gas or candle flame through a piece of cobalt blue glass which transmits light from the two ends of the spectrum chiefly. Hold the glass eight or ten inches before the face and fixate some point on it; the flame will appear pinkish with a blue border. Fixate some point considerably beyond the flame; the flame is now bluish and the border is a fine red line. *b*. Look at the edge of the window frame next the pane, and bring a card before the eye so that about half the pupil is covered; if the card has been brought up from the frame side, the frame will be bordered with yellow; if from the pane side, with blue. In ordinary vision these fringes do not appear, because the colors overlap one another and produce a practically colorfess mixture. *c*. v. Bezold's experiment. Something similar may be observed, on regarding the parallel lines of the left figure under Ex. 111 with imperfect accommodation.

Cf. Helmholtz, *op. cit.* G. 156-164; Fr. 172 (125)—179 (131). Beaunis, Nouveaux éléments de physiologie humaine, II, 506. v. Bezold, v. Graefe's Archiv f. Ophthalm., XIV, Heft 2, 1-29.

105. Accompanyments of accommodation. *a*. Notice that as the subject in Ex. 103 accomodates for a near point, his pupil grows smaller, and as he accommodates for a far point, grows larger. Cf. also Ex. 106, *b*. Degrees of accommodation suitable for objects at different distances are habitually associated with the amounts of convergence of the lines of sight necessary to fix the eyes upon such objects, and a little practice is necessary before the convergence and accommodation can be dissociated. Place a couple of postage stamps six inches apart on the table and look at them from a distance of twelve or fifteen inches with crossed eyes so that the left eye looks at the right stamp and the right eye at the left stamp; the lines of sight now cross only a few inches from the eyes and the accommodation is for that distance and not for the true distance of the stamps, as is betrayed by the blurring of their images. Holding a pencil at the crossing point of the lines of sight is helpful in first attempts at crossed vision.

Cf. Helmholtz, *op. cit.* G. 130, Fr. 142 (104).

106. Entoptic phenomena: *Muscae volitantes*, etc. Fix a lens of short focus at some distance from a bright gas or candle flame. *a*. Set up in the focus of the lens a card pierced with a very fine hole, bring the eye close to the hole and look toward the light; the eye should be far enough from the hole to prevent the edge of the lens from being seen; the rays of light that now reach the eye are divergent and the crystaline lens does not bring them to a focus on the retina, but only refracts them to such a degree that they traverse the eye nearly parallel and thus in suitable condition for casting sharp shadows upon the retina of objects on or in the eye. The lens will appear full of light, and in it will be seen a variety of shadings, blotches and specks, single or in strings, the outward projection of the shadows just mentioned. The figures in this luminous field will vary from person to person, even from eye to eye, but in almost every eye some will be found that move and some that remain fixed and only move with the eye. Of the moving figures some are due to particles and viscous fluids on the surface of the eye; they seem to move downward and are changed by winking. Notice for example the horizontal bands that follow a slow dropping and raising of the upper

lid. Others, the *muscae volitantes* are frequently noticed without any apparatus; they appear as bright irregular threads, strings of beads, or groups of points, orsingle minute circles with light centres. They seem to move downward in the field and consequently actually move upward in the vitrious humor where they are found. Of the permanent ones, some are due to irregularities of structure or small bodies in the lens and its capsule (spots with dark or bright centres, bright irregular lines, or dark radiating lines corresponding probably to the radial structure of the lens); others of a relatively permanent character can be produced on the cornea by continued rubbing or pressure on the eyeball. *b*. The round spot of light in which these things are seen represents the pupil, and the dark ground around it the shadow of the iris. Notice the change in the size of the spot of light, as the eye is accommodated for different distances (cf. Ex. 105), and as the other eye is exposed to, or covered from, the light. The change begins in about half a second. It shows the close connection of the iris mechanisms of the two eyes and is typical of the way in which the two eyes co-operate as parts of a single visual machine. Some of these entoptic observations may be made with a pierced card alone, or simply by looking directly at a broad expanse of clear sky with out any apparatus at all.

Cf. Helmholtz, *op. cit.* G. 184-192 and Tafel I. which represents the appearance of several of the entoptic objects; Fr. 204 (149)—214 (156) and Pl. V; also pp. 548 (419)—558 (427).

107. Retinal blood-vessels, Purkinje's vessel figures. *a*, Concentrate a strong light, (prefarably in a dark room) or even direct sunlight, with a double convex lens of short focus on the sclerotic in the outer corner of the eye of the subject, requesting him to turn the eye toward the nose and giving him a dark background to look toward. Make the spot of light on the sclerotic as small and sharp as possible and give to the lens a gentle to and fro or circular motion. and after a little the subject cannot fail to see upon the field which the light makes reddish yellow the dark branching figure of the shadows of the retinal vessels. Notice that the area directly fixated, is partially surrounded, but not crossed by the vessels. In this lies the yellow spot (*macula lutea*) or area of clearest vision of the retina, not, however, to be observed in this experiment. The centre from which the vessels radiate lies in the point of entrance of the optic nerve. In this form of the experiment the light radiates in all directions within the eye from the illuminated point of the sclerotic. *b*. Somewhat the same kind of an image of the vessels is to be secured by moving a candle about near the eye, below it and a little to one side. In this experiment some indication of the region of the yellow spot is to be seen. In this form of the experiment the light enters by the pupil, forms an image on a part of the retina somewhat remote from the centre and this retinal image is the source of light by which the vessel shadows are cast. *c*. Look through a pin hole in a card directly at the clear sky or any other strongly illuminated even surface or at a broad gas flame. Give the card a rather rapid circular motion and the finer retinal vessels in the region of the yellow spot will readily be seen, among them also a small colored or slightly tinted spot (best seen perhaps by gas light) representing the *macula*, and in its centre a shadowy dot (representing the *fovea* or point of clearest vision) which appears to rotate when the motion of the card is circular. If the card is moved horizontally the vertical vessels alone appear; if vertically, the horizontal vessels. Notice also the granular appearance of the *macula*; the granulations have been supposed to represent the visual cones of that region. The finer retinal vessels can also be seen when looking at the vacant field of a compound microscope, if the eye is moved about rapidly. In all of these cases it is important that the shadows be kept moving; if they stand still, they are lost. The expla-

nation is partly physiological, the portions of the retina on which the shadows rest soon gain in sensitiveness enough to compensate for the less light received, and partly psychological, moving objects in general being more readily attended to, and those whose images rest continously on the retina without motion being particularly subject to neglect. Once having become acquainted with the appearance of these vessel figures it is often possible to see traces of them without any apparatus. Parts of them, with something of the projection of the yellow spot, may sometimes be seen for an instant as dark figures on the diffusely lighted walls and ceiling or as light figures on the dark field of the closed eyes when the eyes are opened and closed after a glance at the window on first waking in the morning, or in blue when looking at the snow and winking on a bright morning, or projected on the sky and keeping time with the pulse after a rapid walk up hill.

Helmholtz, *op. cit.* G. 192-198. Fr. 214 (156)—221 (161).

108. Retinal circulation. Look steadily through two or three thicknesses of blue glass at the clear sky or a bright cloud, and observe a large number of what seem to be bright points darting hither and thither like bees in a swarm or rapidly blown snow-flakes. Careful observation will also establish that the bright points are followed by darker shadowy ones. Pick out a speck on the window to serve as a fixation point, look at it steadily and observe that while the movements of the points seem irregular the same lines are retraced by them from time to time. When several of their courses have been accurately observed, repeat the experiment for demonstrating the finer retinal vessels (Ex. 107 *c.*) and notice that fine vessels are found which correspond to the courses which the points seem to follow. These flying points can be seen without the glass by a steady gaze at an evenly lighted bright surface, and some times a rhythmic acceleration of their movement will be found, corresponding to the pulse. Helmholtz explains the phenomenon as due to the temporary clogging of fine capillary vessels by large blood corpuscles. The bright lines (the apparent tracks of bright points) are really the relatively empty capillary tubes ahead of the corpuscles, which, after an instant, are driven onward by others crowding behind and in turn give the shadow that apparently follows the bright points.

Cf. Helmholtz, *op. cit.* G. 198; Fr. 221 (837), 555 (425), Rood, American Journal of Science, 2d Series, XXX, 1860, 264-265, 385-386.

109. The Blind-spot. Mariotte's experiment. The point of entrance of the optic nerve is unprovided with visual end-organs and is irresponsive to light. *a.* This insensitiveness is easily demonstrated with the diagrams below. Close the left eye and keeping the right fixed on the asterisk in the upper diagram move it backward and forward till a point is found where the black oval disappears. For the blind spot of the

left eye use the second diagram. The blind spot may be demonstrated simultaneously in both eyes by the use of a figure like that below enlarged a couple of times. The experimenter should look at the asterisk while he holds a sheet of paper in the median plane of his head, to prevent each eye from seeing the other's part of the diagram. *b.* To draw

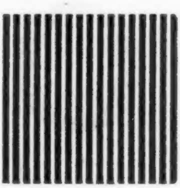
the projection of the blind-spot, arrange the head support described above, place opposite the face, at a distance of about 18 inches, a vertical sheet of white paper, and put a dot on it for a fixation point. Fasten upon the end of a light rod a bit of black paper about 2 mm. square or blacken the end of the rod with ink. Bring the face into position, close one eye, and fix the other upon the dot. Move the rod slowly so as to bring the little square over the part of the white paper corresponding to the blind spot, dotting on the paper the points where the square disappears or reappears. Repeat at various points till the outline of the projection of the blind spot is complete. If the mapping is carefully carried out, the map will probably show the points of departure of the large blood vessel, that enter with the nerve.

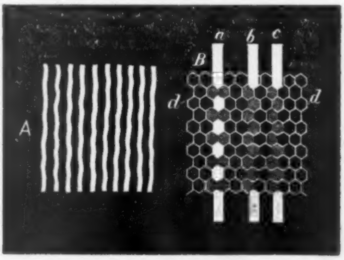
Helmholtz, *op. cit.* G. 250-254, Fr. 284 (210)—289 (214).

110. The yellow spot, *macula lutea*. The projection of the yellow spot in the visual field can be made visible in several ways. Two have already been mentioned in Ex. 107; others are as follows. *a.* Close the eyes for a few seconds and then look with one of them through a flat sided bottle of chrome alum solution at a brightly lighted surface (not yellow) or the clear sky. In the blue green solution a rose colored spot will be seen which corresponds to the yellow spot. The light that comes through the chrome alum solution is chiefly a mixture of red and green and blue. The pigment of the yellow spot absorbs a portion of the blue and green and transmits the rest, which makes a rose colored mixture, to the visual organs behind it. *b.* The region of the yellow spot may be seen as an area of somewhat deeper shade when the eye looks at an evenly lighted surface like the ceiling, and the illumination is made intermittent by moving the spread fingers to and fro between the eye and the ceiling.

Cf. Helmholtz, *op. cit.* Fr. 548 (419)—551 (421). On *a.* cf. Maxwell, On Color-vision at different points of the Retina, Report of the British Assoc., 1870; or Vol. II, pp. 230-232 of Maxwell's Scientific Papers. Cambridge, 1890.

111. Visual cones in the *fovea*. Bergmann's experiment. Place the left hand diagram in a good light and look at it from a distance of a





yard and a half or two yards. Observe the apparent bending and beading of the lines. This is supposed to be due to the mosaic arrangement

of the visual cones. The cones that are touched by the image of one of the white lines are stimulated in proportion as they are more or less touched. Those that are much stimulated furnish the sensation of the white line and its irregularities, those that are little stimulated join with those that are not touched at all to give the image of the black line and its irregularities. This is schematically represented in the right hand cut.

Cf. Helmholtz, *op. cit.*, G. 257-258, Fr. 293 (217)—294 (218). Bergmann, Zeitschrift für rat. Med., (3), II, 88.

112 Acuteness of vision, *minimum visibile*, and size of the cones in the *fovea*. Place the parallel line diagram used in the last experiment in a good light and walk backward from it till the lines can just no longer be distinguished as separate. If the experimenter's eyes are not normal he should use glasses that fit his eyes for distinct vision at the distance required. Measure the distance between the eye and the diagram and calculate the angle whose apex lies in the crossing point of the lines of direction (about 7 mm. back of the cornea and 16 mm. in front of the retina) and whose base is the distance from the middle of one line of the diagram to the middle of the next; in this diagram 1.58 mm. This angle measures the least visible extent when discrimination is involved; the least luminous extent that can still impress the retina is far smaller, as witness the visibility of the stars. On the supposition that if the sensations of two cones are to be separable they must be separated by an unstimulated, or at least by a less stimulated, cone, it has generally been considered that the cones could not subtend a greater angle than that found in this experiment, 60″—90″, representing 0.004—0.006 mm. on the retina, and this agrees well with microscopical measurements. But as Helmholtz notices (Phys. Opt. 2nd ed. p. 260) this experiment does no more than prove that there are on the retina rows of sensitive elements the middle lines of which are separated by the angular distance found in the experiment. The elements themselves, if properly arranged may be somewhat larger. Calculation of the number of such elements in a sq. mm. of the retina, based on this view of the experiment agrees well in the case of Helmholtz's own determination with the result of microscopical counting. *b*. The discriminative power of the retina falls off rapidly in all directions from the *fovea*, more rapidly above and below than in a horizontal direction. Arrange a head rest and perpendicular plane as in Ex. 109 *b*. Place upon the end of the rod used in that experiment a card on which have been made two black dots 2 mm. in diameter and 4 mm. from centre to centre. Move the card horizontally toward the fixation point, beginning beyond the point at which the two dots can be distinguished and moving inward till they can just be distinguished. Measure the distance from the fixation point and repeat several times both to the right and left of the fixation point and above and below, holding the card so that both dots are in each case equally distant from the fixation point.

Helmholtz, *op. cit.*, G. 255—264, Fr. 291 (215)—301 (223).

113. Mechanical stimulation of the retina. *a*. Phosphenes. Turn the open or closed eye as far as possible toward the nose and press on the eye-lid at the outer corner with the finger or the tip of a pen holder. On the opposite side of the visual field will be seen a more or less complete circle of light surrounded by a narrow dark band, outside of which again is a narrow band of light. Notice the color of the light seen. Get the phosphenes by pressure at other points of the eye ball. *b*. Press the eye moderately with some large object, say the angle of the wrist when the hand is bent backward, and continue the pressure for a minute or two. Peculiar palpitating figures will be observed and

strange color effects. The former Helmholtz compares to the tingling of a member that is "asleep." *c*. Standing before a window, close the eyes and turn them sharply from side to side. As they reach the extreme position in either direction observe immediately in front of the face a sudden blue spot surrounded by a yellow band. A second fainter spot farther from the centre in the direction of motion may also be seen. The yellow ring is due to the stimulation of the portion of the retina in the region of the blind spot in the eye that turns inward. The blue spot represent the blind spot in the same eye. Cf. explanation in the latter part of Ex. 115.

Helmholtz, *op. cit.* G. 235-239, Fr. 266 (196)—270 (200). Le Conte, American Journal of Psychology, III, 1889-90, 364—366.

114. Idio-retinal light, light chaos, light dust. Close and cover the eyes so as to exclude all light, or experiment in a perfectly dark room. Let the after effects of objective light fade away and then watch the shifting light clouds of retinal light. The cause of the retinal light is not altogether clear, but it is supposed to be a chemical action of the blood on the nervous portion of the visual apparatus. Aubert estimates its brightness at about half the brightness of a sheet of paper illuminated by the planet Venus when at its brightest. *b*. When awake in the night time in a room that is almost perfectly dark (e. g. in which the form of the window and the large pieces of furniture cannot be made out) notice that the white clothing of the arms can be seen faintly as they are moved about, but not when they are still. In the last case the very faint light they reflect is not sufficient to make them distinguishable from clouds of idio-retinal light.

Cf. Helmholtz: *op. cit.* G. 242-243, Fr. 274 (202)—275 (203). On *b*. cf. Helmholtz, Die Störung der Wahrnehmung kleinster Helligkeitsunterschiede, Zeitschrift für Psychologie, I, 1890, 6-9.

115. Electrical stimulation of the visual apparatus. Moisten throughly with strong salt water both the electrodes and the portions of the skin to which they are to be applied. Place one of the electrodes on the forehead (or on the edge of the table and lay the forehead upon it), the other on the back of the neck; or, if the current is strong enough, hold it in the hand or lay it on the table and put the hand upon it. At each opening or closing of the circuit a bright flash will be seen, whether the eyes are closed or open. With the eyes closed and covered the effects of the continuous current may be observed. In this case it is well to apply the electrode slowly and carefully so as to avoid as much as possible the flash caused by the sudden closing of the circuit. When the positive electrode is on the forehead, the negative on the back of the neck a transient pale violet light will be seen distributed generally over the field and forming a small bright spot at its centre. Sometimes traces of the blind spot appear. The violet light soon fades and on opening the circuit, there is a notable darkening of the field with a momentary view of the blind spots as bright disks. When the negative electrode is on the forehead, the positive on the back of the neck, the phenomena are in general reversed, the darkening occuring on closing the circuit, the violet light on opening it. Helmholtz sums up these and other experiments as follows: "Constant electrical circulation through the retina from the cones toward the ganglion cells gives the sensation of darkness, circulation in the contrary direction gives the sensation of brightness." (Phys. Opt. 2nd ed. 247). That the blind spot should appear as a disk of different color from the rest of the field seems to be due to the fact that the sensitive parts of the retina immediately surrounding it are somewhat shielded from the electric current, and as usual their condition is attributed to the blind spot also. The experiment is not entirely a pleasant one, on account of the feeling

which the current produces in the head, the electrical taste in the mouth and the reddening of the skin under the electrodes.

Cf. Helmholtz, *op. cit.* G. 243-248, Fr. 275 (203)—281 (207).

116. After-images, accidental or consecutive images. After-images in which the relations of light and shade of the original object are preserved are called *Positive After-images*. Those in which these relations are reversed (as in a photographic negative) are called *Negative After-images*. *Positive after-images* are of changing colors, but most important to notice here are those of the color of the object (*like colored*), and of the complementary color (*opposite colored*). *Negative after-images*, so far as observed, are always *opposite colored*. All after-images, especially the positive, can best be observed in the morning when the eyes are well rested. *a*. Negative after-images: look steadily for a minute at a fixed point of the window, then at a white screen or an evenly lighted unfigured wall; the dark parts of the window will now appear light and *vice versa*. Get a lasting after-image and look at a corner of the room or at a chair, or other object of uneven surface; notice how the image seems to fit itself to the surface upon which it rests. After a little practice it is also possible at desire to see the image floating in the air instead of lying on the back-ground. *b*. Look steadily at a bright colored object or some bits of colored paper, then at the screen; observe that the colors of the after-images are approximately complementary to the colors of the objects producing them. Negative after-images are some times very lasting and for that reason are those most frequently noticed in ordinary experience; they are a phenomenon of retinal fatigue. *c*. Positive after-images. Look for an instant (one-third of a second) at the window, then close and cover the eyes, or look at a dark surface; for a very short time an after-image like the original object in color and distribution of light and shade can be seen. The positive after-image is of short duration and is not so readily observed as the negative; it is a phenomenon of retinal inertia, of the prolongation of retinal excitation. *d*. Colored positive after-images. Look for an instant at a gas flame through a piece of red glass, then close the eyes and observe the red image; repeat the experiment continuing the fixation of the flame for half a minute; the resulting after-image will be bright as before but of the complementary color. *e*. Get an after-image of the window of not too great an intensity, and alternately project it on a sheet of white paper and the dark field of the closed and covered eyes; it will be found negative on the white back-ground and positive on the dark. *f*. Get a good after-image of the window and observe with closed and covered eyes the play of colors as the image fades. Try several times and observe that the order of succession is the same.

Cf. Helmholtz, *op. cit.* Fr. 446 (338), 471 (357)—500 (380). Wundt, Physiologische Psychologie, 3rd ed I, 472-476.

117. Effect of eye-motions on after-images. Get a moderately strong after-image of the window; look at the wall and keep the eyes actively in motion; the image will be seen with difficulty while the eye is in motion; when the eye is brought to rest, however, it will soon appear. In general any visual stimulus that moves with the eye is less effective than one that does not.

Cf. Exner, Das Verschwinden der Nachbilder bei Augenbewegungen. Zeitschrift für Psychologie, I, 1890, 47-51.

118. The seat of the after-image. An after-image due to exclusive stimulation of a single eye may under proper conditions sometimes seem to be seen with the other unstimulated eye. From this it has been inferred that the seat of after-images was central, not peripheral; that is, in the visual centres of the brain, not in the

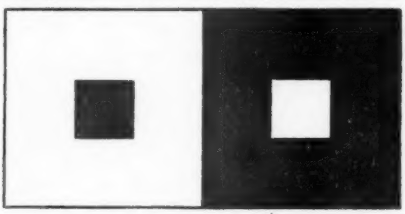
eye. The following experiments show, however, that the after-image is really seen with the eye first stimulated, and so render the hypothesis of a central location unnecessary. *a.* Look steadily for several seconds at a bit of red paper on a white ground, using only one eye, say the right, and keeping the other closed; when a strong after-image has been secured, remove the paper, close the right eye, open the left and again look steadily at the white ground; after a little the field will darken and the after-image will reappear. If the red does not produce a sufficiently lasting image, substitute for it a gas flame or some other bright object. That we have really to do, however, with the eye originally stimulated, (its present dark field being superposed upon the light one of the other eye) appears from the results of *b* and *c*. *b.* Get the after-image as before; then open both eyes and bring a bit of cardboard before the eyes alternately; bringing it before the left eye rather brightens the image; bringing it before the right dims or abolishes it; the image is therefore chiefly affected by what affects the right eye. *c.* Get the after-image again and close and cover both eyes; observe the the color of the after-image as projected on the dark field; then open the left eye, letting the right eye remain closed and covered; the after-image will be seen, not in the color it has when the right eye is open and the image is projected in the light field, but in that which it has in the dark field of the closed eye.

Cf. Delabarre, On the seat of Optical After-Images, American Journal of Psychology, II, 1888-89, 326-328.

119. **After-images of motion.** Fasten upon the rotation apparatus a disk like that in the first cut on page 475. Then look at a page of print or into the face of a by-stander and notice the apparent shrinking (if the spiral has seemed to run outward) or swelling (if the spiral has seemed to run inward). Illusions of increase or decrease of distance sometimes accompany those of motion. These after-images of motion have been explained as due to unconscious persisting movements of the eyes. This is probably incorrect, for in the present case it would seem necessary that the eyes should move in all directions at the same time.[1]

Cf. Helmholtz, *op. cit.* Fr. 766 (603)—769 (605). Bowditch and Hall, Optical Illusions of Motion, Journal of Physiology, III, 297-307. Mach, Bewegungsempfindungen, Leipzig, 1875, pp. 59-61, (see also pp. 61-65 for yet another kind of after-image) and Analyse der Empfindungen, Jena, 1886, pp. 65-67.

120. **Irradiation.** This term is used to designate the apparent enlargement of bright surfaces at the expense of adjacent dark surfaces. It is most strongly marked when the bright surface is intense and the accommodation is imperfect, but is not absent with perfect accommodation. Even with perfect accommodation, and much more so with im-

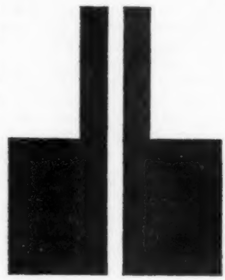


perfect accommodation, the line of juncture of a bright and dark surface is not really a sharp line but a narrow band of gray of which more than

[1] My assistant Mr. T. L. Bolton, has noticed that these after-images are subject to illusory transference like those of Ex. 118.

the proper amount is credited to the white, for reasons to be brought out in the section to follow on the Psychophysic Law. The following are some of the common cases of irradiation: *a*. Hold a ruler or a straight edged piece of black card-board close before a gas or candle flame so as to cover a portion of it, and notice that the flame seems to cut into the edge, and if there are differences in brightness the brightest parts cut in deepest. *b*. Notice that the white squares in the diagram below, when brought into a strong light, seem larger than the black, though they measure the same in size.

c. Irradiation of dark lines. A black line on a white surface (or a white line on a black surface) may some times be enlarged by the greater part of its gray fringe, because near the outer edge of the fringe the blackness (or for white lines, the whiteness) decreases very rapidly and so seems to make a boundary. Look at the accompanying diagram through a lens that will make accommodation very imperfect. The narrow black strips will appear larger for the reason just mentioned, while the lower black areas will be cut into as in the ordinary cases of irradiation, giving to the white stripe between the shape of a club with the handle uppermost. Helmholtz suggests with reason that these two phenomena, having quite different causes, should have different names, and the term "irradiation" be confined strictly to such enlargement of white surfaces as takes place with exact accommodation.



Cf. Helmholtz, *op. cit.*, G. 394-400, Fr. 425 (321)—433 (327).

121. Reflex movement of the eye. The eye is a moving as well as a seeing member and its motor functions are of great importance for psychology. Of the first importance is the constant reflex tendency of the eye to move in such a way as to bring any bright image lying on a peripheral part of the retina, or any to which attention is directed, into the area of clearest vision. Many evidences of this tendency will be found in the ordinary course of vision. By way of experiment, try to study attentively a *musca volitans* or a negative after-image that is just to one side of the direct line of sight. The apparent motion of the object measures the energy of the reflex.

122. Associated movements of the eyes. The two eyes form a single visual instrument and even when one eye is closed it follows to a considerable degree the movements of its open companion. *a*. Close one eye and, resting the finger-tip lightly on the lid, feel the motions of the eye as the other looks from point to point of the visual field. *b*. Get a monocular after-image as in Ex. 118 and when it has become apparently visible to the open eye, notice that it seems to accompany that eye as it takes one fixation point after another in the field of regard.

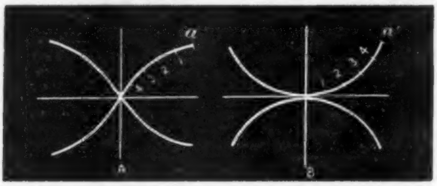
123. Motions of the eyes when the lines of sight are parallel, Donders's and Listing's laws. All motions of the eye can be interpreted as rotations of greater or less extent about one or more of three axes: a sagittal axis, corresponding nearly with the line of sight; a frontal axis, extending horizontally from right to left; and a vertical axis. All these intersect in the centre of rotation of the eye. Now it is easily conceivable that for any position of the line of sight, *e. g.* 15° to the right and 10° upward, there would be an infinite number of positions that the eye might assume by rotation about the line of sight itself. As a matter of fact, however, it does not assume an indefinite number of positions, but one and only one, no matter by what route the line of sight may have come to that point. This is the *law of constant orienta-*

tion or Donders's law. *Listing's law* goes further and asserts that the position is not only fixed, but is such as the eye would assume if the line of sight were moved from its primary position (approximately that in which the eye looks straight forward to the horizon) to the point in question without any rotation at all about the line of sight, but about a fixed axis standing perpendicular at the centre of rotation to both the primary and the new position of the line of sight. The advantage to vision of the constancy of orientation and the exclusion of rotation about the line of sight is considerable, especially in determining directions in the field of regard. The correctness of these laws is easy to demonstrate. *a.* Donders's law. Cut in a sheet of black cardboard two slits an eighth of an inch wide and six or eight inches long, crossing at right angles. Set the cardboard in the window or before some other brightly lighted surface. Arrange a head rest at some distance and when the head is in position, get a strong after-image of the cross, fixating its middle point. Then, without moving the head, turn the eyes to different parts of the walls and ceiling. The image will suffer various distortions from the different surfaces upon which it is projected, but each time the eye returns to the same point the image will lie as before. If the wall does not offer figures by which this can be shown, have an assistant mark the position of the image upon it. The after-image is of course fixed on the retina and can move only as the eye moves. *b.* Listing's law. Make over the cross used in *a* into an eight rayed star by cutting two other narrow slits across its centre. Arrange the card before a brightly lighted wall and parallel to it at a height a little less than that of the eyes when the head is in position. Draw lines or stretch threads on the wall that shall appear to continue the rays of the star upward and right and left, and downward if convenient. Fix the head rest directly before the star at a distance of five or six yards or more. Adjust the head so that when the after-image of the star is carried along the horizontal or vertical line its corresponding ray will coincide exactly with the line. When this condition is fulfilled for both lines the eyes and lines of sight are in the *primary position*. When the primary position has been found, carry the after-image along the lines prolonging the other rays and observe that as before the after-image of the ray coincides with its line. This would be found true, for all except extreme positions, of all other rays, and shows that the eye does not in such motions rotate about the line of sight. *c.* In motions from other or secondary positions, however, there is such a rotation. Turn the head somewhat to one side or tip it forward or backward from the primary position, repeat *b* and notice that the lines of the after-image betray some rotation.

Cf. Helmholtz, *op. cit.*, Fr. 601 (462)—610 (470), 621 (479) ff. Le Conte, Sight, pp. 164-177.

124. Actual movements of the eyes. Rapid motions of the eyes are not executed with mechanical exactness according to Listing's law, though it gives correctly the end position reached. The axis of rotation is not quite constant and the lines passed over by the point of sight are therefore not quite straight. This is easy to observe as follows. In a dark room turn down the gas till it burns in a flame not more than 8 or 10 mm. high. Then using this as a point of departure in the primary position look suddenly from it to other points of fixation in various directions about it, and notice the shape of the long positive after-images that result from the motion of the image of the flame, over the retina. These will probably have the shape of the radii in the left hand figure below. The newest part of the after-image is that next the light, the oldest part is that next the fixation point, for example at *a*. If the points of the after-image curve are now interpreted in the order of time, it

appears that the eye at first moved rather rapidly toward the right but rather slowly upward, while at last it moved rather slowly toward the



right and rapidly upward. Plotting the curve accordingly we get the reverse curve shown in *B* which shows the true track of the fixation point. It is said that for some eyes the after-images, though curved, do not coincide with those figured in *A*.

Cf. Wundt. Beiträge zur Theorie der Sinneswahrnehmung, Leipzig, 1862, pp. 139 ff. 202. Hermann's Handbuch der Physiol. III, Th. 1, 450-451.

125. Convergent movements of the eyes. When the lines of sight converge, the movements of the eye do not follow Listing's law. When the lines of sight converge in the primary position both eyes rotate outward; as the lines of sight are elevated, the convergence remaining the same, the outward rotation increases; as they are depressed, the rotation diminishes and finally becomes zero. On a sheet of cardboard draw a series of equi-distant parallel vertical lines one or two inches apart and eight or ten inches long, drawing the left half of the group in black ink, the right half in red. Cross both sets midway from top to bottom by a horizontal line, red in the red set and black in the black set. Fasten the cardboard flat upon a vertical support and arrange the head rest in front of it. The horizontal line of the diagram should be on a level with the eyes. *a*. Fasten a bit of wire vertically between the eyes and the diagram in such a way that it can be moved to and from the eyes. Bring the head into position and look at the wire, but give attention to the diagram. It will be seen that the red and black lines are not quite parallel and that they are less nearly so as the wire is brought nearer the face. The red lines (seen by the left eye) seem to incline a little toward the right and the black lines (seen by the right eye) toward the left. As the wire comes near and the convergence is great the horizontal lines will also show the rotation. This apparent rotation of the lines is not, as in the case of the after-images, a sign that the corresponding eye has rotated in the way that they have, but that it has rotated in the opposite way. *b*. Repeat this with the head much inclined forward (the equivalent of elevating the eyes) and with it thrown far back (equivalent of depressing the eyes) taking care that the wire is always at the same distance from the eyes. In the first case the apparent rotation of th lines is increased, and in the second decreased to zero or even transformed into rotation in the opposite direction.

Cf. Helmholtz. *op. cit.* Fr. 609 (469)—610 (470). Le Conte, Sight, 177—191. Hermann's Handbuch der Physiol. III, Th. 1, 496 ff.

126. Involuntary movements of the eyes. Lay a small scrap of red paper on a large piece of blue. Fixate some point on the edge of the red. After a few seconds of steady fixation, the color near the line of separation, will be seen to brighten, now in the red and now in the blue. This is due to the small unintentional movements of the eyes.

LETTERS AND NOTES.

LONDON, April 8, 1892.

TO THE EDITOR.

It been suggested to me that a short account of the various opportunities which exist in London for the study of Philosophy in its different branches, embracing Psychology, Logic and Ethics, would be acceptable to your readers. I have therefore put together a few notes showing what is done at the various Institutions in the Metropolis which make the teaching of any of the branches of Philosophy a systematic part of their work. The account does not claim to be exhaustive and may not even do full justice to some of the Institutions named, though it seeks to do this as far as published materials permit. The information is derived partly from personal knowledge and partly from the calendars and printed syllabuses, and if not very complete it may be interesting to Americans who are devoting so much attention to philosophical studies.

Naturally one begins with the University of London which may be presumed to be the chief influence in directing the line of study followed in the London Colleges. In Mental Science the influence of James Mill, and later Grote and Bain as examiners, did much to fix the schedule of study. It may not be universally known to readers of your Journal that the University of London is an examining and not a teaching University. Its graduates come from all kinds of colleges and they may have been prepared by private instruction. Its degrees are valued for their high standard and the severe tests, which it is admitted, they impose. For the B. A. and B. Sc. pass degress, a very fair knowledge of Psychology, Logic and Ethics was requisite until recently. Now it is optional whether the candidate takes Mental Science or Mathematics. There is a separate examination for Honours in Mental Science; for this, in addition to the above subjects, special books are set each year. The M. A. degree (Branch III) to which graduates in Arts may proceed, provides however the chief Mental Science examination. This includes Logic, Psychology and Ethics, Political Economy, History of Philosophy and Political Philosophy. For the latter two divisions, special books are set each year; for the other subjects no books are prescribed by the University. Science Graduates may proceed to a D. Sc. Degree in Philosophy by a further examination for which an original thesis must be produced.

The University also conducts an examination in the Art, Theory and History of Teaching, for which it confers a Teacher's Diploma; this examination includes a paper on Mental and Moral Science. The present examiners in Mental Science are Dr. James Sully, so well known by his writings on Psychology, and Professor Knight of St. Andrew's University.

Coming now to the teaching Institutions, University College (Gower Street) deservedly stands quite to the front in any estimate of philosophical work. Its students prepare for the University with which the college has been closely associated from its foundation. This is, I be-

lieve, the only institution in the Metropolis in which there is any endowment of Philosophy.

Professor Croom Robertson, M. A., the pupil and discriminating disciple of Bain, has for more than twenty years filled the chair of Grote Professor, and numbers among his students many of those who now lecture elsewhere, including the present writer. Professor Robertson is widely known for his philosophical erudition, his cultured lectures, his scientific spirit, and his devotion to the cause of philosophy. His are the only lectures of note on Philosophical Systems and History, which are available to the general public in the Metropolis. Professor Robertson was also the Editor of "Mind" from its inception in 1876 until last year when his health unfortunately compelled him to discontinue that task. The method pursued by Professor Robertson in his Lectures will be best understood from a copy of his general Syllabus extracted from the College Course.

GENERAL COURSE.

Psychology:—Thirty Lectures in *First* Term, beginning October 12th.

Logic:—Thirty Lectures in *Second* Term, beginning January 11th.

General Philosophy; Ethics:—Twenty Lectures in *Third* Term, beginning May 2nd.

The course is primarily designed to meet the requirements of Elementary Students, and more particularly Candidates for the B. A. and B. Sc. Degrees of the University of London. The topics to be treated under the head of GENERAL PHILOSOPHY correspond with some of those included in the *psychological* and *logical* divisions of the University's scheme of Mental and Moral Science. To make the instruction as thorough as possible, lectures are varied or supplemented by conversation, and are followed up by a regular series of exercises to be written at home.

Students who take the Course with a view to B. A. or B. Sc. Honors or to the M. A. Degree, (Branch III) or for no purpose of examination at all, have their respective needs carefully attended to from the first, their reading being specially directed, and (where necessary) more advanced exercises being prescribed.

SPECIAL COURSES.

HISTORY OF PHILOSOPHY.

First and Second Terms.

PLATO (*Theaetetus Republic*) and HUME, as prescribed for the M. A. Degree in 1892.

Third Term.

PLATO (*Phaedo*) and ADAM SMITH (*Moral Sentiments*), as prescribed for B. A. and B. Sc. Honors in 1892.

These Special Courses will be given at times to be arranged privately with the Students concerned. Names for the M. A. Course should be sent in to the Professor by the 16th October; for the B. A. and B. Sc. Honors Course, by the end of the Second Term.

The *John Stuart Mill Scholarship* is open to the competition of Students within two Sessions *after* completion of the General Course.

The subject prescribed for the Mill Dissertation in the Session 1891-92 is "THE DEVELOPMENT OF ENGLISH PHILOSOPHY TILL HUME."

King's College, (Strand), has also been in close relation with the University from its foundation though it now sends up very few students for Degrees. It is a Church of England College and confers a title of its own (A. N. C.). There are regular courses in Logic and Mental

Philosophy, though Philosophy is not now a very prominent feature of the College Course. The Rev. Frederick Denison Maurice was at one time professor here. The following is a copy of the Syllabus as it appears in the College Calendar:

LOGIC AND MENTAL PHILOSOPHY.—"Lectures are given on these subjects on Wednesdays from two to three P. M. Each course will run through three terms, will consist of about thirty lectures, and is intended to give such a general knowledge of the subject as every educated man may be suspected to possess. The requirements of the B. A. and B. Sc. examinations of the University of London will be constantly kept in view." The Rev. A. Caldecott, St. John's College, Cambridge, is the present professor. There is an evening class on Mondays from seven to eight P. M. The College has also a separate department for ladies at Kensington where a Course of Lectures on the Ethical teaching of English Poets and Essayists of the Nineteenth Century is being given.

Bedford College for Ladies, (Baker Street, W.,) is an institution for the higher education of women. Founded in 1849, it has regularly prepared its students for the University of London since 1879 when the Degrees were thrown open to women, and its curriculum is mainly regulated by the requirements of the University. The College is well appointed and supplied with laboratories and apparatus. The accommodation is excellent and some twenty-five students reside on the premises. A Training Department for Teachers has been recently formed; for this class as well as to the Students for Degrees, Mrs. Bryant, D. Sc., (London), is delivering a course of Lectures on the elements of Psychology; this will be followed by a course in Ethics and Logic.

There are several valuable Evening Colleges in London which provide higher education for persons occupied during the day, and which also prepare for the examinations of the University. Foremost amongst them is the *Birkbeck Institution*, (Chancery Lane), with some four thousand students of both sexes, and classes and lectures on all kinds of subjects from Arithmetic to Astronomy. The writer has for many years lectured here on Logic, Psychology, Ethics, and Political Economy, to numbers of students engaged during the day as City Clerks, Teachers, etc. Some proceed to Degrees at the University, and others to the Cambridge Higher, Women's and other examinations. The principal text books are Jevons, Mill, Bain and Veynes, on Logic; Sully and Höffding on Psychology, and Sidgwick's Methods and History on Ethics. The courses are arranged to cover all the London University examinations in these subjects.

The *City of London College*, (Moorfields), is an exactly analogous Institution. Its curriculum resembles that of the Birkbeck, and with a smaller body of students it carries on work of the same character. Logic, Psychology and Ethics have a permanent place in its Syllabus, and the description just given may be taken as indicating its character and aims.

A comparatively new Institution is the *London Ethical Society*, (Essex Street, Strand), established about five years ago with the object of developing interests in Ethical and Social subjects. This is done primarily by free Sunday evening Lectures followed by discussions; the current programme of Lecturers contains among others the names of Allanson Picton, Felix Adler, and D. G. Ritchie. The Society further organizes courses of week night lectures on Ethical and Philosophical subjects in which it aims at "establishing more systematic teaching in the subjects dealt with at the Sunday lectures." Mr. Muirhead lectures on Ethics. Mrs. Bryant has recently completed a course of Lectures on "Mind and Life." Mr. B. Bosanquet is now engaged on a course on "The Nature of Knowledge," and it is proposed next winter to deal with "The Philosophy of Art," and "History of Religion."

There is another Institution which must be noticed. *Toynbee Hall*, Universities Settlement in the east of London, is probably well known to Americans. There, for some years, a colony of university men has been working in many ways to elevate the tone of east London; the plan has included lectures, reading classes, students' societies, etc. Many eminent men have delivered lectures on Ethical topics, and a Toynbee Philosophical Society has been founded over which various able University men have from time to time presided, including recently Mr. Alexander of Lincoln College, Oxford, whose name is known as a writer on Ethics and Psychology. The labor is of a voluntary kind for the most part, and its primary aim had doubtless much more of a missionary character, though it has developed considerably.

It remains only to speak of the work of Dr. James Sully as a Lecturer on Psychology and Education. For many years Dr. Sully has lectured at the College of Preceptors (Bloomsbury) on the "Science, Art and History of Education." These lectures are the most popular and systematic lectures of the kind in the Metropolis; they attract annually a large attendance of Teachers in Secondary Schools. Their aim as set forth in the the Syllabus is to show that "there are definite truths relating on the one hand to the characteristics and laws of growth of the child, and on the other hand to the ends of human life which have a direct bearing on the Teacher's work."

Dr. Sully's reputation as a Psychologist is too great to need mention here, his books have become text books throughout the world; his experience in the application of Mental Science to the principles and practice of Education is equally extensive with his scientific acquirements in the field of Mental Science, and these lectures are a most important factor in the diffusion of the principles of Psychology in the Metropolis.

Dr. Sully lectures also on Psychology at the Maria Grey Training College, Fitzroy Street, the first Training College for Women Teachers in Secondary or "High Schools." This College was founded in 1878 for the Training in the Theory and Practice of Education of women who desire to devote themselves to teaching in girls' Secondary Schools, and who aim at a University Certificate of professional skill. Dr. Sully further lectures at some of the Normal Schools for Elementary Teachers.

The following very condensed summary of a current Syllabus will give some idea of the plan adopted in Dr. Sully's lectures on Education:

1. Education as a science and an art—its place in relation to human activity, social progress, civilization.

2. The true purpose of education—different conception of the aim or bearing upon perfection, fitness, happiness, knowledge, and moral character.

3. How the educational end is to be realized—by exciting normal reaction in the organism—by self activity, bearing of education upon natural development.

4. Physical education as an end and as a process—healthy development of powers—games, gymnastics, discipline.

5. Education of senses—training of mind organs—awakening of intellect, attention, observation, object teaching, perception—naming and registering results of observation.

6. Transition from sense perception to ideation—image, ideas, naming and reproduction, memory, realization of the unseen—constructive imagination, language and description.

7. Transition from concrete to abstract—generalization—process of thought—definition and induction—aids in mental development from analogies; reasoning.

8. Psychological and logical view of knowledge—order of acquisition—empirical as introductory to scientific knowledge—topical concatenation of studies.

9. Knowledge and particular knowledge—selection of studies and bearing upon development of human faculty—the ideal currriculum.

10. Education as concerned with feelings—calling forth interest by education, enthusiasm, æsthetic culture—formation of taste in literature and in art.

11. Education as acting on will and character—value of method—development of intelligent sense of duty—influence of custom, law, society, etc.

12. Typical plan of education and its concrete modification—the spirit of the age—nationality—adjustment of education to individual needs—specialization, etc.

This brief review of Philosophical Teaching in London takes no note of Societies for discussion like the Aristotelian, or of instruction which is more or less for a private character, or of the University Extension Society, which, except in the cognate branch of Political Economy, has not yet developed Philosophical Study, although the lectures of the Ethical Society have been brought into relation with this Society. It will be seen that in a scattered and disconnected form there is a considerable supply of instruction in some of the branches of Philosophy, under the heads of Logic, Psychology, and Ethics, and to these might be added Political Economy did the scope of the paper permit, but for the study of General Philosophy on a systematic plan, University College is practically the only centre.

The subject of a Teaching University for London has for several years been agitating the public mind. A recent attempt to transform University College, King's College, and the Medical Colleges into such a University to the exclusion of the other teaching institutions has failed. A Royal Commission is being appointed to consider the whole subject and to suggest some plan co-ordinating under one head the scattered agencies of a higher education in the Metropolis. It is confidently hoped that before long some system will be devised by which they will be brought into closer relation with the existing University, or failing that, be organized under a New Teaching University which will systematize their work, stimulate to the utmost their energies and prevent that waste of power which is inevitable in the circumstances when a number of isolated educational bodies follow their own plans with no common bond or directing force.

We may hope that when this project takes definite shape, among other good results will be an impetus to philosophical study, and the full recognition of its bearin g upon education and life. And further we may hope that a Metropolitan University would establish and properly equip a laboratory for experimental psychology and research, such as is to be found on the Continent and in more than one American college, the absence of which can not but be regarded as indicating a very imperfect appreciation of the value of such pursuits in the greatest city in the world.

I am,

Yours faithfully,

G. ARMITAGE SMITH.

COPENHAGEN, April 6, 1892.

TO THE EDITOR,

My dear Sir,

You have in your friendly letter expressed the wish to know something about my work as a teacher of philosophy. There is not much to tell, but perhaps it will be of interest to you and your readers

to hear something about the manner in which philosophy and psychology are studied in our little country.[1]

We have the rule at our university, that the students in their first year go through a philosophical course, consisting of four hours per week through two semesters. My colleague, Professor Kromoa, and I conduct this course, so that the students can choose which of us they will hear. In this course I make use of my "Outlines of Psychology," of which the greater part is read every year. I treat Psychology as a fundament of Philosophy, all three great philosophical problems—the problem of knowledge, the problem of being and the ethical problems—being intelligible only from the point of view of human consciousness. Empirical Psychology is thus an introduction to Philosophy. In this spirit is my book conceived. I have endeavored to give a complete view of the facts and forms of psychological life with special stress on the subjects which are interesting from a universal philosophical point of view. I have, so far as possible, endeavoured to make use of all the sources of psychological experience and knowledge. And I have sought to express my thoughts as briefly and clearly as possible. I cannot here omit to say that the friendly reception my book has experienced in England and America, is in a very great measure due to the excellent English translation, for which I am indebted to Miss Mary Lowndes.

In the said course I give further the elements of Logic (after the method of Jevons), and sometimes a few chapters of Ethics or of the history of Philosophy.

Other lessons are designed for those students that are peculiarly interested in Philosophy. Here I discuss philosophical and psychological questions, often in the form of colloquia. Thus I have treated a series of questions on which of late I have written several papers, which in a German translation have appeared in the "*Vierteljahresschrift f. wiss. Philosophie* (vol. XIII—XV). Or I read with my students some philosophical work (e. g. the Ethics of Aristotle; Spinoza's "Ethica;" Kant's Kritik der reinen Vernunft, der praktischen Vernunft, der Urtheilskraft; the Logic of Stuart Mill; Spencer's "First Principles;" Wundt's Logic).

Finally, I lecture on the history of Philosophy or on Ethics. These lectures are attended not only by students, but also by other ladies and gentlemen. The rooms of our University stand open for all who think they can get any profit from the lectures.

In these last years (after the appearance of my "Ethics") my studies have been concentrated on the history of Philosophy.

My colleague, Prof. Kromoa, has as text-book for his course his "Logic and Psychology" (of which a German translation has appeared). His other lectures are on the theory of knowledge (on which he has written a work, which is translated into German: "Das Naturerkennen") and Pedagogics. Dr. Wildens is lecturing on Aesthethics and Sociology, Dr. Starke on Ethics, Sociology and History of Philosophy. We have at our University a psychological laboratory, under the direction of Dr. Lehmann, whose experimental treatises are translated in Wundt's Studien.

This will give you a short view of my "work and surroundings."

Believe me, dear Sir, yours very truly,

HAROLD HÖFFDING.

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[1]For a fuller view see the paper of my late young friend *Knud Ipsen:* "Die dänsche Philosophie des letzten Jahrzehnts" (Phil. Monatshefte, 1891, XXVII, 290.) and my paper in the Archiv fur Geschichte der Philosophie, II.

BERLIN, April 14, 1892.

My dear Professor Stanley Hall:

I feel rather flattered by your kind inquiry about my lectures, but find it difficult to give you an appropriate answer in a short letter. Since I have come to the University of Berlin (1888), I have lectured on subjects which have always specially interested me. The physiology of protoplasm, comparative physiology, general physiology, psychogenesis (in the Victoria Lyceum), physiology of sensation (in the Urania), and macrobiotics.

The titles of my regular lectures in the University as given in the catalogue do not convey an exact idea of their full contents, but I am obliged to give short titles like all the others. This does not in the least prevent me from expounding my own views, for instance on psychology in my lectures on the physiology of hypnotism, the first academical lectures on the subject held in Germany, on the evolution of physiological functions in general and on psychical functions especially in my lectures on "Concurrency" ("die Lehre vom Kampfe um das Dasein.")

Protoplasm being the basis of life is my greatest favorite, and I have been led to investigate this wonderful complex of changing substances with increasing interest, but the necessity of preparing new editions of previous books (Mind of the Child, 4th ed.,) and pamphlets or papers, absorbs a good deal of my time, or rather has done so during 1889–91. I am working hard now to get my work on the organic elements and the generic system of elements in general ready for print. I hope to see you in London August 1, at the Psychological Congress, and at Edinburgh August 3, at the meeting of the British Association. I shall read a paper at London on the origin of the notion of numbers, and send some abstracts, which, although they are not quite new, may be unknown to you and may perhaps interest you.

Yours sincerely,

WM. PREYER.

Monsieur le Prof. G. Stanley Hall, Editeur de L'AMERICAN JOURNAL OF PSYCHOLOGY, *Clark University.*

GENÈVE, SUISSE, 14 Avril, 1892.

CHER MONSIEUR,

Une chaire extraordinaire de *Psychologie Expérimentale* a été créeé l'an dernier dans la Faculté des Sciences de notre université (mais sans laboratoire). Ayant été chargé de cette enseignement, j'ai naturellement jugé indispensable de le compléter par des travaux pratiques. Au moyen de quelques instruments que je possède, et d'une salle que l'État m'a prêtée dans le bâtiment de l'universite, j'ai pu, le 15 février dernier, ouvrir aux étudiants un laboratoire très modeste, dont nous devons encore nous contenter cet été. Pour le semestre d'hiver prochain, nous avons la perspective d'obtenir un local mieux aménagé, composé de cinq petites chambres d'une superficie totale d'environ 100 mètres carrée, où je mettrai les instruments suivants à la disposition des étudiants: chronoscope de Hipp (de Peyer et Fararger à Neuchâtel); chronomètre de d'Arsonval (de Verdin à Paris); pendule marquant les .01 de seconde (d'Elbs à Fribourg ¹|B); quelques instruments d'optique et d'acoustique, périmètre de Landolt, diapasons d'Appun, etc; divers modèles du cerveau, entre autres le grand modèle d'Auzoux (décrit dans votre journal, tome IV, p. 132); enfin quelques-uns des ingénieux instruments imaginés par M. Münsterberg à Fribourg ¹|B, et que grâce à son obligeance j'ai pu faire reproduire par son constructeur M. H. Elbs.

(Sphygmograph, Augenmaasapparat, Schallapparat, Arbwemegungen-apparat.) Quand nous aurons ainsi installé en fait un petit laboratoire de psychologie, l'État ne pourra manquer de le reconnaitre officiellement, et de lui accorder un crédit annuel permettant de lui donner peu à peu un plus grand developpement. En ce qui concerne nos travaux, nous nous bornons pour le moment à quelques recherches élémentaires sur les temps de réaction et d'association, sur les types d'imagination, etc.

Je me souviendrai à l'occasion, cher Monsieur, de votre offre aimable d'insérer de nos travaux dans votre estimé et très-intéressant Journal, dont je suis un fidèle abonné, et un lecteur régulier, depuis sa fondation.

Veuillez, je vous prie, recevoir l'expression de la considération la plus distinguée

de votre bien dévoué,

THÉODORE FLOURNOY.

DORPAT, 11. IV. 1892.

Hochgeehrter Herr College!

In Beantwortung Ihrer geehrten Zuschrift beeile ich mich, die gewünschten Auskünfte Ihnen zu übermitteln. 1. Die Klinik für Nerven- und Geisteskranke verfügt über 80 Betten. 2. Es existirt ein Cabinet a) für mikroskopische, b) für psychophysische Untersuchungen. 3) Ich lese klinische Psychiatrie, 4 Stunden in der Woche, und Poliklinik der Nerven- und Geisteskrankheiten ebenfalls 4 Stunden. Der Besuch der Vorlesungen ist für die Studirenden nicht obligatorisch; auch findet kein Examen statt. 4. Im laufenden Jahre habe ich eine Arbeit, betitelt "Criminalanthropologie" (rüssisch) veröffentlicht. Dr. Daraszkiewicz, Assistent der Klinik, erfasste eine Studie, "Ueber Hebephrenia" (Inauguraldissertation).

Hochachtungsvoll,

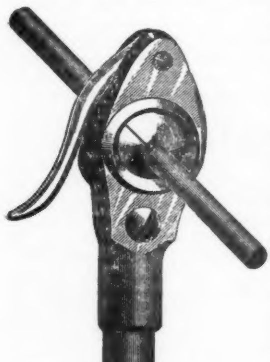
WLADIMIR v. TSCHISCH.

On the Question of Psychophysiology, Consciousness and Hypnotism. In Mind, 1891, XVI, No. 63, E. W. Scripture writes, "The [materialistic] theory asserts that certain of these nervous phenomena produce states of consciousness or mental phenomena and others do not." Materialism is an obscure term. To be sure, such an assertion and a monism understood in this sense would be nonsensical. Researches in hypnotism, however, have strikingly proved that so far as its reminiscent content is concerned our consciousness (subjectivism) depends directly on the phenomena of inhibition or on the absence of such phenomena. Normal dreamlife, natural and artificial somnambulism, all prove that our cerebral activity can divide itself into several varieties or kinds which may be or may not be separated from their objective dynamisms and which appear subjectively as completely independent of each other. The most familiar form of cerebral activity appears to us in the shape of our chief consciousness in the waking condition. This chief consciousness, however, we can, by hypnotic suggestion, insert or cut out at pleasure. The nervous processes which at each moment appear as "unconscious" are really not such, but are only cut out from the momentary chain of phenomena of the chief consciousness, whether it be that they become obliterated for a time, or that a sudden inhibition hinders them from taking part in an association with that chain of phenomena, or that, as is often the case, they take place in another part of the central nervous system of which the subjectivism is mediately and loosely connected with our cerebral consciousness (just as its cell-fiber system is mediately and loosely connected with the cerebral systems), or finally that the process is so short and so weak that even in occurring it is, so to speak, forgotten. According to this view, which agrees essentially with Janet's and Dessoir's "multiple consciousness," we do not need to assume a nervous process that goes on without consciousness. On the contrary

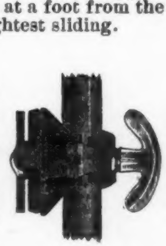
we can and must suppose that in all probability consciousness is not only to be attributed to various parts of the nervous system, but exists also outside of the nervous system in the natural world as the simple primitive form of subjectivism. Physiological psychology has consequently to study the correspondence between the phenomena of our chief consciousness (that is, of the field of psychology) with that part of the cerebral activity to which they correspond and therefrom to deduce the laws for other similar correspondence. It thus has to attempt to reduce the psychological phenomena to the laws of the physiology of the nervous system. PROF. A. FOREL.

Hypnotism in the Asylum. In my work on hypnotism I have mentioned that I used suggestion in the noisy divisions of the Burghölzli Asylum for the purpose of making the attendants insensitive during their sleep for the dreadful racket of the patients; thus they can sleep quietly and restfully and yet wake up upon the occurence of any unusual disturbance among the patients. Up to the present the chief noise was on the women's side and this action was not necessary on the men's side. Last summer, however, two new attendants complained of the great noise of the restless male patients and asked me for help. It was sufficient for me to hypnotise them with the appropriate suggestions last June. Since that time they have not heard the noise during the night and have always slept well, although the noise has continued to be very great. PROF. A. FOREL.

O. C. White of Worcester has patented a ball-joint that is exactly what psychologists and physiologists have so long sought. One of the forms is shown in figure 1, where the joint is fixed at the end of a rod. The fastening of the joint takes place with absolutely no variation of the adjustment. The manner in which the parts clamp together is peculiar, being everywhere a wedge-action. The curvature of the inside of the socket is that of a sphere smaller than the ball which it encloses; likewise the hole through which the slide-rod is placed is of smaller radius than the rod. Consequently the parts do not touch over their whole surfaces, which would render firm fastening difficult, and after a little wear, impossible. As a result a slight pressure on the lever serves to cause a good fastening and with full pressure a 2¼ inch ball will support 75 pounds on the rod at a foot from the center without the slightest sliding.



The clamp shown in figure 2, is also a universal joint, but the movement is obtained on a different principle. It is arranged to slide on the rod of a stand in the usual way but it contains two discs revolving in a vertical plane, these having a cylindrical opening for the rod to be held. This gives the up and down motion, a complete circular movement in any vertical plane and, by pushing the rod through to the desired extent, the radius of the circle can be of any length.



According to the Yale circular of graduate instruction, early in next year laboratory work in experimental and physiological psychology, under a special and competent instructor, will be opened to graduate students,—comprising the following two courses:

1. Experimental and Physiological Psychology. 2 hrs. both terms.

This course will provide for a study (illustrated by charts, models, histological preparations, and a certain amount of laboratory work) of the human nervous mechanism, and of the principal relations which exist between changes in this mechanism and the activities of the mind. The text-book is Ladd's "Elements of Physiological Psychology."

2. Special Problems in Psychology.

Under the guidance, and with the assistance, of the instructor, particular problems in experimental and physiological psychology may be worked out in the laboratory. Such work will be permitted to count for the degree of Doctor of Philosophy, according to its excellence and the amount of well-spent time devoted to it. It is expected also that, in certain cases, theses for this degree may be prepared as giving the results of such work.

The psychological instruction at Harvard next year will consist of three courses. The elementary course will be conducted by Prof. Royce on the foundation of James's Briefer Course; it will extend over one-third of the year with three hours a week. The advanced course will be conducted by Dr. Nichols, using Ladd's Outlines and James's Principles and including a thorough course of laboratory exercises; it will occupy three hours a week throughout the year. The graduate course will be in the hands of Prof. Münsterberg, formerly of the University of Freiburg, who is now Director of the Psychological Laboratory; he will have general control of the experimental work.

The Susan Linn Sage School of philosophy, at Cornell, was founded in September, 1891, with the addition of $200,000 to the previous endowment for a professorship. The leading idea seems to be the employment of specialists in each line of philosophy. One instructor devotes all his time to the history of ancient philosophy, and two others attend strictly to the history of modern philosophy along with systematic metaphysics. There is a professor of the history and philosophy of religion and another of pedagogy. Another professor gives most of his time to ethics; there is also a special instructor of logic. *The Philosophical Review* edited by Prof. Schurman is the organ of the school. A department of psychology has also been opened. The laboratory will contain equipment of apparatus most of which was made at Leipzig under the personal supervision of the professor, Dr. Angell.

The following list of lectures and exercises in the German universities last winter is intended to include not only the strictly experimental work but also those courses in pathology that have a distinctly psychological bearing.

Leipzig: WUNDT, (with the assistance of Dr. Külpe), Special investigations and exercises in the psychological laboratory. KÜLPE, Lectures on psychology, Introductory course in the psychological laboratory. GLÖCKNER, Pedagogical psychology. FLECHSIG, Psychiatrical clinic, Forensic psychiatry. *Berlin:* DILTHEY, On the application of psychology to pedagogical questions, Lectures on psychology. LAZARUS, Lectures on psychology. EBBINGHAUS, Lectures on psychology with reference to experimental and physiological psychology, Exercises in experimental psychology. JOLLY, Pathology and therapeutics of men-

tal diseases, Clinic. *Bonn:* MARTIUS, Elements of psychology, Psychological exercises. PELMAN, Mental disturbances that border on insanity, Clinic. KOCHS, On hypnotism, sleep and the narcotic condition. *Goettingen:* G. E. MÜLLER, Lectures on psychology, Experimental psychological investigations. MEYER, Psychiatrical clinic. *Heidelberg:* KRAEPELIN, Physiological psychology, Psychiatrical clinic.

The *Institut Psycho-Physiologique de Paris* was founded in 1891 for the theoretical and practical study of the psychological and therapeutical applications of hypnotism. A free clinic for nervous diseases is annexed to the institute where physicians and students of medicine regularly inscribed are admitted for practice in psychotherapy. A private hospital adjoining the clinic receives morphiomaniacs and those requiring constant attention.

The *Société d' hypnologie* of Paris has monthly meetings, at which papers on hypnotism are read and clinical cases presented. Dr. Dumontpallier is president and Dr. Bérillon is general secretary.

The Second Session of the International Congress of Experimental Psychology will be held in London, on Tuesday, August 2d, 1892, and the three following days.

Arrangements have already been made by which the main branches of contemporary psychological research will be represented. In addition to the chief lines of investigation comprising the general experimental study of psychical phenomena in the normal human mind, it is intended to bring into prominence such kindred departments of research as the neurological consideration of the cerebral conditions of mental processes; the study of the lower forms of mind in the infant, in the lower races of mankind, and in animals, together with the connected laws of heredity; also the pathology of mind and criminology. Certain aspects of recent hypnotic research will also be discussed, and reports will be given of the results of the census of hallucinations which it was decided to carry out at the first session of the Congress (Paris, 1889).

On Feb. 12th died Hermann Aubert; born November, 1826, at Frankfort a. O., he studied chiefly in Berlin, taking his degree in 1850. Appointed Professor of Physiology at Rostock in 1865, he devoted himself mainly to physiological optics. After a smaller work, the *Physiologie der Netzhant* (1865), he published his famous "*Physiologische Optik*" (1876). His last experimental investigations referred to the limits of accuracy of ophthalmometric measurements.

The publication of an encyclopedia of medical propædeutics is about to be begun under the direction of Prof. Gad in Berlin. Extensive space is to be given to physiological psychology and the neighboring subjects. The psychology of the senses, excluding sight, is to be treated by Goldscheider, physiological optics by Cl. Dubois-Reymond, cerebral physiology by Ziehen and general psychology by Münsterberg.

At the University of Basel died the eighty year old Prof. I. Hoppe, formerly practicing physician, but for many years busied with psychological speculation. He has bequeathed half a million marks to the university, with the condition that a commission shall be appointed which shall in Hoppe's house meditate uninterruptedly on the nature of the soul, and shall in publishing their results refrain from the use of all foreign words.

The laboratory of experimental psychology of Columbia College is established in four rooms, occupying the upper floor of the president's house. These include rooms for instruction and research, and a dark room for the study of vision. A collection of apparatus has been secured at a cost of about $2,500, and this will be further increased during the

present year. The liberal regulation recently adopted by the trustees makes it possible for men of science not connected with the college to use the laboratory and apparatus for special research.

According to statistical material gathered by Volkelt, the lectures on psychology have increased most of all in the universities of German tongue. During the year 1887-88, the lectures were distributed as follows: Psychology 67, logic 64, metaphysic 25, ethics 41, history of philosophy 154; whereas in the year 1890-91, there were: Psychology 78, logic 64, metaphysic 27, ethics 51, history of philosophy 138.

A free course of lectures on the clinical and medico-legal applications of hypnotism is given by Dr. Bérillon in the Practice School of the Faculty of Medicine at Paris.

Dr. Max Bessoir, well known for his writings on hypnotic subjects, has been appointed Docent of Philosophy at the University of Berlin. He will lecture chiefly on psychology and æsthetics.

Professor Flournoy has been appointed to the newly founded chair for physiological psychology at the University of Geneva. A laboratory is about to be begun on the plan of that at Freiburg.

The psychiatrist and psychophysicist, Prof. Kräpelin, who was recently called from Dorpat to the medical faculty of Heidelberg, has begun a laboratory for experimental psychology. Psychological lectures will be given in addition to the psychiatrical ones. The University of Dorpat will continue the courses in psychology under the direction of Kräpelin's successor, Woldemar von Tschisch, the Petersburg psychiatrist. Both Kräpelin and Tschisch are former pupils of Wundt.

The third Congress of Criminal Anthropology will be held at Brussels from the 28th of August to the 3d of September of this year. The extensive program includes nineteen groups of subjects to be considered. Communications are to be addressed to M. C. Dr. Semal, président, l'Asile de Mons, Belgique.

A grant of £250 has been made to the Physiological Laboratory of the University of Cambridge, England, for the purpose of establishing a psychological department. This is the only opportunity for psychological instruction in the laboratory in England.

A laboratory has been established in the University of Toronto. Its fittings have cost about $450. An appropriation of $800 has been made for apparatus, and $300 a year have been allowed as for maintaining it. Prof. Baldwin has gone to Europe largely in the interests of equipment.

Prof. Frank Angell of Cornell has accepted the chair of psychology at the Stanford University.

Dr. Edward Pace, a pupil of Wundt, who took his degree at Leipzig with a dissertation entitled "Das Relativitätsprincip in Herbert Spencer's psychologischer Entwicklungslehre," has taken the chair of psychology in the Catholic University at Washington and has started a laboratory.

Edmund Delabarre, Ph. D., (Freiburg), has just gone to Brown University after taking his degree with an experimental investigation made in Münsterberg's laboratory. A laboratory will be started.

Dr. Charles Strong will lecture next fall at the University of Chicago.

American workers are to be again warned against importing apparatus which is wholly or partly made of wood. In a few months the American climate warps the wood and consequently renders the apparatus, in most cases, almost useless. This fact, long since known to piano-dealers, is sometimes lost sight of in the laboratories or is not learned till after expensive experience.